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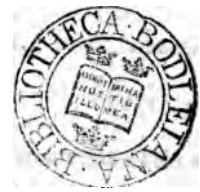

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No. XLIII. SECOND SERIES. Dec. 1805.

Specification of the Patent granted to JOSEPH BRAMAH, *of Pimlico, in the County of Middlesex, Engineer; for sundry Improvements in the Art of making Paper.*

Dated April 25, 1805.

TO all to whom these presents shall come, &c. NOW KNOW YE, that in compliance with the said proviso, I the said Joseph Bramah do hereby declare, that my said invention is described and ascertained in manner following; that is to say: First, I perform the office of moulder of the sheets by machinery, which may be conducted by persons not possessing the skill of those competent to perform this operation in the usual way, and by which means the following points will be ascertained, and on which I rest the merits and novelty of this part of my invention, which I call the first class of my said improvements, *viz.* By this method or application of machinery sheets of much larger dimensions can easily be made much more equal in their weight and thickness, and with greater expedition, than can possibly be done by hand in the usual way. To perform this, I cause the

dimensions of the vat, in which the stuff is usually put for moulding, to be made in length and width exactly that I intend the sheet shall be, and its depth may be of any measure not less than eighteen or twenty inches. This vat or cistern I call a regulating reservoir, and in which there is a frame or rim of wood, made to slide up and down at pleasure, by means of any kind of apparatus by which it may be lifted up and depressed at pleasure, with its upper surface perfectly level in all directions by means of an axis working through a stuffing box, or otherwise, in the end or side of the said reservoir, so that the frame may be lowered or lifted up by a handle or winch, externally applied; and the motion of the frame being so limited, that when up at its highest extremity its upper surface may be exactly on a level with the brim or upper edge of one side of the reservoir, which side must be made a little lower than the other three; and when the said frame or rim is depressed to its lowest extremity it is a few inches below the said side or brim on which it was when up upon a level. The paper-mould is also nicely fitted to the inside of the reservoir on all sides, and is rested or laid upon this frame or sliding rim, so that it may be lifted up or depressed in a perfect horizontal position when the frame is lowered or raised for that purpose, so that the mould and frame may always uniformly move together, whether up or down. On some part of the side or end of the said reservoir, near or at the bottom thereof, is cut an aperture, of tolerable large dimensions, through which the water with which the reservoir is filled when in use can quickly discharge itself. This aperture is cased or covered over with a hollow wooden trunk on the outside, and is fitted to the external side of the cistern with a water-tight junction, and continued up about one quarter or half an inch above the

the surface of the paper-mould within the reservoir when said mould is depressed to its lowest station. At this line of altitude the top of this external trunk or waste pipe is cut horizontally, or has a vertical hole made in its side; the lowest extremity of which must cut the same line as above, so that the internal water may always be gauged by this discharge exactly to the same level in either case, that may occasionally be adopted to accommodate local circumstances. This mouth or opening in the trunk is covered or closed by a lid or valve, which opens when the mould within the reservoir is raised, and shuts again as it descends by the action of the same machinery. This being thus equipped, there is another cistern placed on one side of the reservoir already described, of any shape or dimensions that may be best adapted to the situation, and having its bottom near on a level with the brim or upper surface of the former one, which I have called the regulating reservoir, so that the upper one may empty into the lower one; this upper cistern I call the feeder or stuff-cistern, and in which there is fixed what is called the hog, or a small agitator, which is constantly moving in the usual way to prevent the subsiding or unequal mixing of the pulp and water. Between this feeder and the regulating reservoir there is a communication by a trunk or spout, nearly or as wide as the side of the reservoir, at the side or end where it forms a junction with it above the surface of the mould when at its lowest station in the reservoir. The mouth of this trunk or spout is opened or shut alternately by a slider, or other means, forming as it were, when shut, a part of the side where the mould slides as the mould raises up, and is opened again alternately as the mould descends.

The whole being thus organised, and ready for action, the regulating reservoir is filled with water till it dis-

charges at the mouth of the external waste or trunk above described; and then the mould being at its lowest station has the wire surface immersed, as before stated, above one quarter or half an inch below the level of the surface of the water; and the valve or lid which covers the mouth or aperture of the said waste or trunk being then shut, prevents the escape of the water from the interior of the reservoir. And while the machine is in this situation, the sluice which opens the communication between the feeder above the mould is then lifted up, and admits the stuff (the proper temper and consistency of which has been previously adjusted) from the feeding cistern to flow upon the surface of the mould to any limited gage or quantity required for the sheet; and when this quantity has flowed, which is instantly done, the sluice is quickly shut, and then, by the motion of the apparatus, which lifts and lowers the frame and mould, is either more or less gradually raised, as experience may suggest, up to its highest altitude; and by opening the valve or lid of the waste trunk at the instant the mould begins to rise, all the water is instantly discharged from above the mould by passing through the wire into the lower part, and is carried off to the former level of the waste, and is thus prepared for a second depression of the mould; or rather another mould, because the loaded one, when raised up to its highest station, is made to push away on slides to meet the coucher's hand, who furnishes at the same time the alternate mould; and when the mould is again depressed, the valve or lid on the waste being shut, the machine is fitted for a second charge, and thus the process is continued with the utmost certainty and ease.

My second improvement consists in an invention for making paper in endless sheets, of any length or width whatever, by a circulating process, and which is performed

formed as follows, *viz.* I take a wheel or circular frame, composed of three or more rings of thin wood or metal, and of a diameter best suited to the use, say three feet or upwards, and of a width suited to the paper pieces I mean to make. This wheel is mounted on a horizontal axis, just like a water wheel, with a shield on each side of the upper extremity of its periphery, so made to fit the edges of the two extreme rings in a segment form, nearly water-tight, as to prevent the lateral discharge of fluid passing over the wheel. These rings are joined together, and held at their stationary distance, by means of a number of transverse thin bars or ribs just the same as those applied to strengthen a common paper mould; and when this is done, all the face of this wheel, whatever may be its diameter or width, is covered or worked with wire, either of the wove or laid kind, so as to form a complete circular mould, similar to what a common one would represent, provided it could be bent into a circle with both its ends to form an imperceptible junction. When thus equipped and stationed on a frame, as before stated, like a water wheel, and capable of being turned on its axis by hand or otherwise, a feeding cistern, prepared with stuff, as in the former instance, is stationed above the wheel, so as to represent the reservoir which turns a mill, and with a sluice or regulator, by which the stuff, in any required quantity, can be let upon the wheel as the wheel is turning on its axis, either by machinery or otherwise. By this means the stuff is thrown upon the wheel or circular mould at discretion; and while the water is uniformly carried off by falling through the wire, the pulp is left upon the wheel, as in the instance of the common mould, and by this means a constant and an endless process may be kept up to any extent. This I call an universal revolving paper mould;

and,

and, in addition to which, I have fixed, a little above or below the horizontal centre line of the wheel, and opposite to the feeding point, a roller covered with felt; which roller is fixed in contact with the wheel by means of two springs upon the axis of the former, which regulates, and renders uniform and gentle, the contact of their peripheries as they are both turning each other round, and it matters little to which of the axes the primum mobile is applied. This I call the couching roller, because it takes the paper from the mould as they reciprocally turn. There are likewise two other rollers, also covered with felt or woollen cloth, which are placed in firmer contact with each other, and between which the sheet is conducted from the couching roller. These represent the action of a calender, and I call them squeezing rollers; and after the paper has passed through or between them, I station other sets, if I find it necessary, either covered with felt or otherwise, so that the sheet so passing between them may be made as dry as possible. After the paper has thus had the water squeezed out, it is then to be conducted over heated plates, charcoal, or other fires, or passed between the rollers of heated calenders, so that when the paper leaves the machine it may be in a state fit for use. By this means a constant supply of prepared stuff, and a continuance of the machine's action, without intermission, will produce an uniform supply of finished paper proportioned to its speed, which speed nothing but practice and attention to necessary circumstances can determine.

My third improvement consists in my having removed the necessity of employing such a number of presses for the dry work, which is unavoidable in works of even but a tolerable extent, owing to the length of time the paper is obliged to continue in them in a compressed state, and

on

on which account a larger capital is necessary, as well as capacious buildings, for the reception of such presses; both of which objections are removed by my new-invented plan, as I make one press only competent to supply the offices of almost any greater number on the common plan, and this I produce by the following means, *viz.* I construct a press on my patent hydro-mechanical principle, of sufficient power, capacity, and strength, to receive the largest sheets I intend to make, and also competent to withstand the greatest exertion the preparations of each class of paper may require from the press. This press, when so completed, I station in or about the middle of the room or building where the dry press work in a paper-mill is performed, in such way, that when the follower or moving bed of the press is lowered to the extent of its slackening station, the upper surface of it shall be exactly on a level with the floor-line of the room or warehouse where the press is fixed. I then prepare and fix on the floor on each side of the press, to some distance each way, extending in a straight line, a rail-road of wood or metal, similar in its principle and office to other rail-roads used in other works; so that, by means of this, carriages or planks, moving on wheels or castors, may be wheeled into the press on one side, and pressed through it to the opposite, when the process of pressing is completed. When this press is thus equipped for work, I prepare a greater or less number of another kind of apparatus, which I call retainers; at least as many in number as will equal the number of presses each respective manufactory would require on the usual principle. These retainers consist of a top and bottom bed, composed of wood or metal, of sufficient strength to resist the re-action of the paper when the press is slackened from its severest squeeze, and to retain it in its most dense

or

or greatest compressed state for any required length of time, after the grasp of the press has been finally withdrawn. These planks or beds are of a length so as to easily pass between the upright jambs or sides which compose the presses frame; they are also united to each other by four iron bars, similar to those which compose the frame or jambs of the actual press. These bars pass through the lower plank or bed, and are fastened to it with plates and keys, or rivetted heads, so as to present no projection or prominent heads on the under side. The said bars are of a length something less than the altitude of the press between the upper and under bed when the follower is level with the floor; so that the whole machine, with all its load, will pass through the press when sliding or wheeling on the rail-road before described, and which guides it in and out of the press. These four bars, already said to be fastened into the lower plank, pass through the upper one in four mortises, so that the upper bed can be raised or depressed to accommodate a greater or less quantity of paper with which the retainer may be charged, and also to all the said head, to slide or approach nearer to the bottom one, as the power of the press gradually causes the paper to occupy less space. These bars are also prepared with each a series of holes or mortises, as near to one another as they can well be made, for the purpose of receiving keys or wedges above the upper bed, to cause the same to maintain its station in defiance of the efforts of the paper to regain its original space when the power of the press is finally withdrawn, and to accommodate the altitude of the pack of paper, be it more or less. When this retainer is charged with paper on one side of the press, it is by means of wheels or casters that are affixed to the under surface of the lower bed or bottom of the retainer, and running on the

the rail-way, wheeled into its station upon the follower of the actual press; and when the retainer has reached its station, there is cut in the follower, for the reception of those parts of the castors that are prominent to the lower surface of the plank, small segment grooves and notches into which these castors fall, in order that the bottom of the retainer may come in actual contact with the follower, and not then rest upon the wheels. The head of the press is also made as much narrower as to admit the four bars, which compose the sides or jambs of each retainer, to pass it on each side during the upward motion of the press, as the paper is compressed; or, in other cases, four holes are made through the head or upper bed of the press itself. By these retainers the paper may be kept in a compressed state, for any length of time, without interfering with the process of the press, as each bale of paper is removed the very instant the press has done its duty; and, by repassing each retainer back again through the press, the squeeze may be repeated at discretion with little trouble.

My fourth and last improvement consists in having so contrived the drying-house of a paper-mill as to render the raising and taking down those ponderous and cumbersome frames on which the paper is hung unnecessary, and by which improvement I not only enable paper-makers of every description to employ women or children to do the duty of the drying-house instead of men, but also add considerable facility to the process of hanging and rehanging the sheets intended to be exposed to dry. By this means paper-makers will be less dependant on men of unruly behaviour, and heavy salaries; the young and more feeble branches of society will get employment; and considerable savings in the cost will of course be made.

In order to produce these good effects, I pursue the following plan, *viz.* Instead of using those cumbersome and unwieldy apparatus above alluded to, and which I think are denominated tribbles, for the purpose of preparing lines at various heights to hang the paper on, and which are of necessity obliged to be taken down, as well as elevated horizontally, every time they are put in use, I prepare a proper number of frames, made of wood, of light scantling, mounted with lines, rails, or wires, in a manner so as to represent so many frames or clothes-horses, similar to those used by any common laundress, but of a length proportioned to the dimensions of the drying house, which may be divided into two or more rows, so as to leave room and proper isles or passages for the convenience of the operators to hang and rehang the sheets; and the height of the said frames may be equal, or nearly equal, to one half the story in which they are fixed. I then station them at proper distances from each other by means of upright posts, with grooves fitted to the frames, so that each may be slided vertically up and down by means of lines and pullies affixed to each, just like sash-windows that are double hung; so that while one of the frames is sliding up to touch the ceiling of the building with its upper edge, the alternate one may be depressed till its lower edge, or the paper which hangs upon it may come nearly in contact with the floor. These being thus stationed, and governed by lines and balance-weights, as I said before, just like sashes double hung, each can be successively depressed, for the convenience of putting the paper on and off within the reach, or nearly so, of a person's hand while standing on the floor; and by this convenient means even children can work them as well as men; and beginning at one extremity of the room may strip or cover every frame with

ease.

ease. And as each frame is charged by sliding them alternately in contrary directions, or one up and the other down, the whole house may be filled to a much greater amount of paper in the same space than can be done in the usual manner.

In witness whereof, &c.

Specification of the Patent granted to Sir GEORGE WRIGHT, *of Ray Lodge, in the County of Essex, Baronet; for an Instrument or Machine for cutting out of solid Stone, Wood, or other Materials, Pillars or Tubes, either cylindrical or conical, with great Saving of Labour and Materials.* Dated March 30, 1805.

With a Plate.

TO all to whom these presents shall come, &c. NOW KNOW YE, that in compliance with the said proviso, and in pursuance of the said statute, I the said Sir George Wright do hereby declare, that my said invention, of an instrument or machine for cutting out of solid stone, wood, or other materials, pillars or tubes, either cylindrical or conical, with great saving of labour and materials, is described and ascertained in manner and form following; that is to say: The invention, and end of the invention, which is the object of this patent, is to form or cut stone, wood, or other materials, into divers tubes or columns of various diameters, and various other circular work, without the labour and expense of chipping or hollowing, and also by means thereof to form pipes, cisterns, cantilivers, chimney-tops, arches, arches for sewers, Gothic-work, semi-circles, ovals, connection of circles or ovals with inverted curvatures, and all other circular-work. This invention is the application of a saw or saws, or other instruments, in a hole or holes

previously drilled in the stone, wood, or other materials, for that purpose, or of saws or instruments for sawing, working, or cutting, from a centre or centres, or in a tangent, of any given circle or oval.

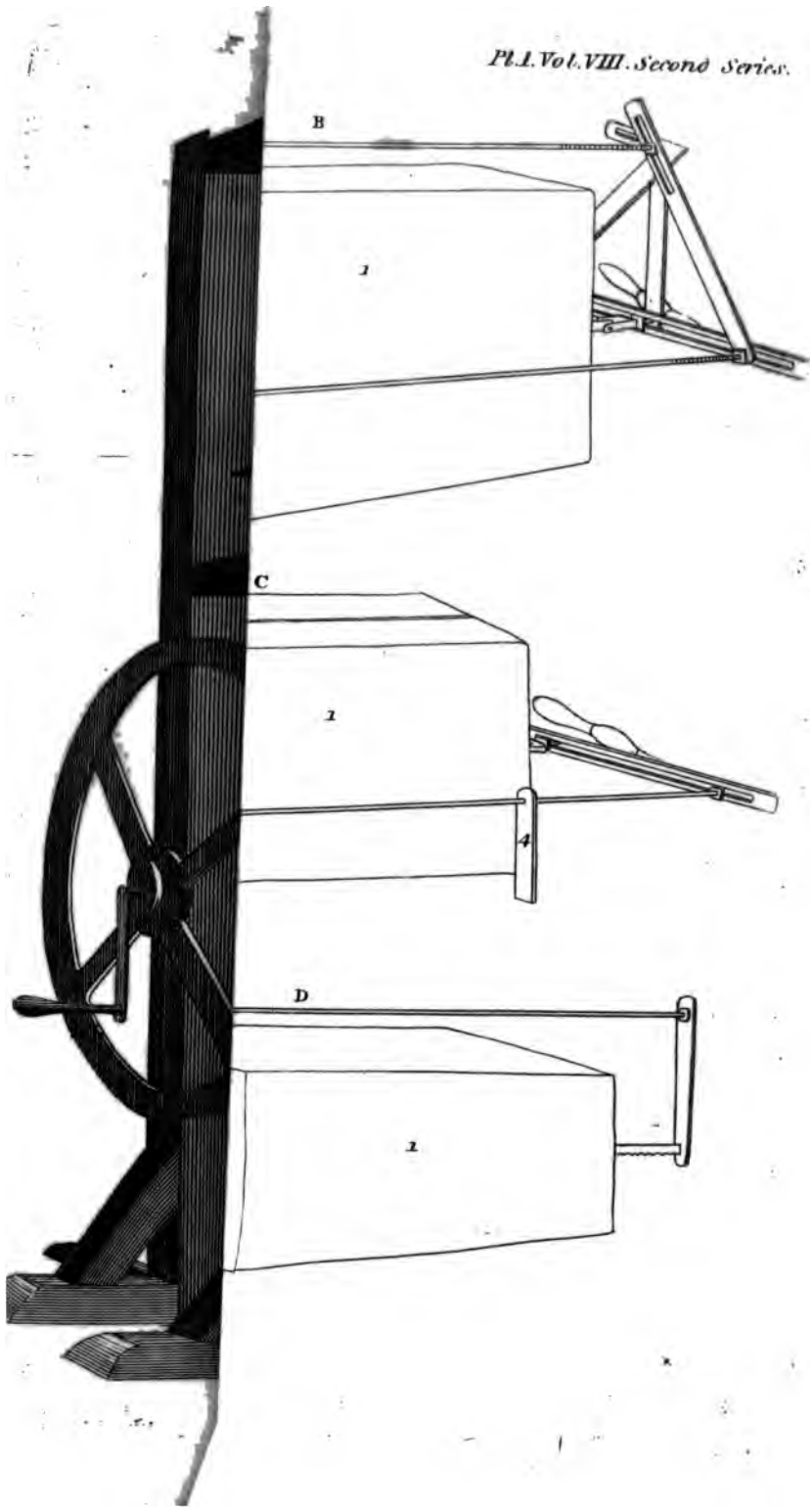
The plan or plate in the margin marked A, (Plate I.) exhibits a block of stone, wood, or other materials, and drill for drilling holes therein. In the first place, a hole or holes is or are drilled through the block, for a centre or for centres, whence the saw is to work, and which hole or holes may be drilled by any other species of drill whatsoever; and other holes, in like manner, are to be drilled at given distances, according to the diameter of the tubes or columns, or other work required to be performed; which holes are to be perforated preparatory to the use or application of the saw, saws, or instruments hereafter described. No. 1, the stone. 3, the shaft, through which the drill 2 runs, having at the same time a rotary and perpendicular motion.

The plans or plates in the margin marked B C D exhibit saws or instruments for working in stone, wood, or other materials, acting on the principle of working in a hole or holes previously drilled therein. No. 1, the stones. 2, the saws.

The plan or plate in the margin marked B exhibits a saw or instrument applied to form the tube or column required, and the manner in which the same acts or performs the work. No. 3, the centre spindle. 4, 4, other cuts.

The plan or plate in the margin marked C exhibits a saw or instrument, by which connective segments of circles are formed from centres, which are out of a block of stone, wood, or other materials. Nos. 3, 3, 3, the connective circles. 4, 4, the moveable centres.

The aforesaid plans or plates in the margin marked B C exhibit two of the modes by which the saws or instruments


B
1
C
1
4
D
1

struments are connected, with their centres acting on one and the same principle, though the assistance of the drill is not required in the plan or plate in the margin marked C.

The plan or plate in the margin marked D exhibits a saw or instrument, acting between circles or grooves fixed at each end of a block of stone, wood, or other materials, without any direct connection with the centre, or drilling any centre hole. No. 3, the rims through which the saw moves.

The plans or plates in the margin exhibit some of the modes, ways, or means, by which this invention may be carried into effect; but other instruments may be made, or employed, or used, to the same effect, and on the same principles.

In witness whereof, &c.

Specification of the Patent granted to Mr. ISAAC HAWKINS, *of Glossop, in the County of Derby; for an Invention applicable to Musical Instruments, the Principles of which are also designed to be applied to various other Machinery. Communicated to him in Confidence by his Son* John Isaac Hawkins, *of Philadelphia, in North America**. Dated November 13, 1800.

TO all to whom these presents shall come, &c. NOW KNOW YE, that in compliance with the said proviso, I the said Isaac Hawkins do hereby declare, that the said improvements were communicated in confidence to me by my son John Isaac Hawkins, residing in Philadelphia, and are described in manner following; that is to say:

* Mr. J. I. Hawkins now resides at Dalby Terrace, City Road, and has also opened a warehouse, No. 96, Golden Square.

First.

First. Metallic elastic strings are to be made, by wire being turned into a spiral, curved, serpentine, or crooked form, or made with links, like a chain, to produce a bass sound, and occupy a much shorter space than is usual.

These strings are also intended to be applied as bands, either single, or two or more sewed side by side, into a piece of cloth, leather, or other substance, for turning lathes, and all other machinery where bands are used for the turning of wheels, &c.

These elastic strings are also designed to be fixed to saddles and stirrup straps, to render them elastic, and thereby hinder the jolting of horses from affecting the riders. It is likewise intended to combine a number of these elastic strings together, and apply them to coaches, and all other carriages where springs are used.

Secondly. Gut or silk, or other strings, are to be rendered water-proof, by being impregnated with drying oils; or gums or other drying substances dissolved in oils or other menstrua, designed to resist the action of the atmosphere, by which strings are frequently put out of tune. These water-proof strings are to be applied to all musical instruments where they can be used.

Thirdly. The strings of piano fortes, and all other finger-keyed instruments, to be fixed in a perpendicular, or nearly perpendicular position, extending from about three or four feet high to within a few inches of the floor. The object of this is to make piano fortes, &c. in a more elegant and convenient shape than any heretofore made.

Fourthly. One end of each string, of all stringed musical instruments, is to be connected with a spring; or a fuzee or levers, to which a spring is fixed, so that the spring shall act with equal force or weight on the string when it contracts or expands.

It is intended to apply this principle, with a little variation, to the equallizing of any irregular power, such as the action of winds, or other irregular power, upon machinery: the spring in this case will be a barrel spring. The irregular power is to act upon the spring to wind it up, while the spring spends its force regularly, or at pleasure, on the movements it is designed to work.

Fifthly. For the purpose of tuning the strings of musical instruments, there are to be sliding frames or carriages, each worked by a screw, in a frame of metal, to which frame or carriage a string is attached. In this also, which may be termed the primary, are one or more secondary sliding frames or carriages; to each of which a string is attached, worked also by a screw in the primary carriage, so that two, three, or more strings may be stretched at the same time, by turning the screw of the primary carriage; and they may be put in tune, one with the other, by turning the screws of the secondary carriages.

Sixthly. There is to be attached to harps, and all other instruments where it will apply, a roller, worked by a pedal or pedals, having pins or other projections, acting upon levers, which press on any of the strings at the pleasure of the performer, in order to vary the tones by shortening their vibrating part.

Seventhly. There is also to be affixed to instruments from which the tones are produced by hammers, a roller, made to turn with considerable velocity, having projecting parts, to strike either immediately or mediately by levers, on the tail of the hammer, to make the hammer continue striking the strings so long as the finger-key is kept down. This may be called the Poiotonic stop.

Eighthly. It is intended to make piano fortes, harps, violins, and indeed all musical-stringed instruments, without

without any box or hollow space under the sound-board, or board which vibrates from the motion of the strings, or, in other words, the sound-board is to be exposed on both sides.

Ninthly. It is designed to draw a lengthened tone from a string or bell, or other vibrating body, by the revolution of a hoop or ring; on the inside of which hairs are stretched, in equal chords of the circle, intersecting each other, forming a regular polygonic elastic surface of hair.

Tenthly. In all instruments where the tone is produced by hammers striking strings or bells, or other bodies, there are to be introduced between the hammers and sounding bodies, gradually at the pleasure of the performer, by a pedal or otherwise, pieces of leather, or other substances, the parts of which are of different degrees of density, thickness, or hardness, so as gradually to vary the tone from loud to soft, or *vice versâ*, by the hammers striking through the medium of the hard, soft, thick, or thin parts of the said pieces.

Eleventhly. The frame which contains the finger-keys of piano fortes, organs, &c. is to turn on pivots or axes, to shut up, so as to contract the space which the instrument occupies when not in use.

Twelfthly. A piece of machinery, worked with a pedal, is to be attached to a stand on which a music-book is placed, to turn over the leaves at pleasure, while the hands are employed otherwise. This may be done several ways; but as the thing is intirely new, the inventor claims it in every possible manner in which it can be done, of which the following plan is one: *viz.* A number of pieces of metal, or other substance, turning on an axis, nearly under or over where the back of the book is put. Each of these pieces has a long arm, with a forked end,

end, taking hold of a leaf of the book; and a short arm, to which a wire is fixed, communicating with the pedal. When the pedal is pressed down it turns the short arm, by means of the wire, and thereby the long arm, which carries the leaf with it. The pedal then disengages itself, and communicates with the arm holding the next leaf, so that there may be so many leaves turned over as there are arms turning on their axes.

In witness whereof, &c.

Specification of the Patent granted to BENJAMIN BATLEY, *of Queen-street, in the City of London, Sugar Refiner; for a Method of refining Sugars.* Dated July 8, 1805.

TO all to whom these presents shall come, &c. NOW KNOW YE, that I the said Benjamin Batley, in compliance with the said proviso contained in the said letters patent, do hereby describe and ascertain the nature of my said invention to be, as is hereinbefore set forth, a new and improved method of refining sugars with milk, and that the process is to be performed in manner following; that is to say: First charge the sugar-pans with the usual quantity of lime-water; and for each ton weight of sugar to be refined allow at the rate of ten gallons of skimmed-milk, (more or less, as may be necessary, according to the quality of the sugar). Mix five gallons of such milk with the lime-water, then skip the sugar, and, after the sugar is skipped, let it remain in the pans till next morning, when the whole, so mixed, is to be stirred together; and, after taking off the first scum of the sugar, add more skimmed milk, and continue to repeat the same, after each scum of the sugar, until the liquor is perfectly cleared.

In witness whereof, &c.

On the Improvement of certain Soils, and cultivating of Waste Lands.

By Mr. THOMAS BEST, *of Hazelbury, near Crewkerne.*

From the COMMUNICATIONS to the BOARD of AGRICULTURE.

HAVING ninety-three acres of land (which I hold under Edward B. Portman, Esq. for ninety-nine years, or determinable on three lives) that had lain for a great number of years in a very uncultivated state, I was determined to try, if possible, to improve them. My neighbouring farmers endeavoured to dissuade me from it, saying, that I should never see principal or interest. The improvement however has answered my utmost wishes, of which the following statement will, I presume, be a sufficient proof. These lands, since the memory of man, were let at 18*l. per annum:* from this sum they increased, owing to the advance of the times, to 22*l.* 5*s.* which is the most they ever let for. In the year 1788 I began on a field of eight acres, by employing some men to take up brambles, furze, and other natural incumbrances, with which two parts in three of that field were covered; in the winter I had it under-ground drained. This, I think, is the first step which a farmer ought to take before he converts his land to tillage. I laid the top turf on two shoulders, about sixteen inches deep, leaving a channel open under, which got the land very dry, and so it continues. The expense of draining, when I first began, was about 30*s. per* acre, but now it is near 40*s.* In December, 1790, I ploughed the whole of this field, and in the February following spread about ten tons of well-mixed dung and earth over every acre. In April I sowed

sowed it with flax: this is a crop that requires, (particularly in strong lands, which this is, being on a strong clay, with some spots of flinty gravel,) in its early part, at least twelve hours rain every week. Unfortunately for me, we had not half rain enough: consequently that crop failed. I then made as good a fallow as the nature of the land and the season would permit. At Michaelmas, 1791, I sowed it with wheat, which produced about eighteen bushels (single Winchester) *per* acre. As soon as the ground was cleared of the wheat, it was ploughed and sown to vetches, of which I had a great crop: I fed these off with sheep in the spring 1793, and the ground which they cleared by day they lay on at night. After the vetches were all eaten, I made a very good fallow, and in September dressed it over with ten hogsheads of lime (which cost 1*s.* 6*d.* *per* hogshead) *per* acre, which I had in the month of May mixed with the head-lands well together. About Michaelmas I sowed it with wheat again, and had too great a burden; for in the month of May, 1794, I was obliged to have a man, with a keen reap-hook, to cut off all the luxuriant blades, the ear not having made its appearance. This precaution, however, did not fully answer my purpose, for a great part of the crop was thrown, notwithstanding I had full twenty-five bushels per acre. I then made a fallow, and in the spring 1795 sowed the field with white oats, and grass-seeds of different sorts, *viz.* rye-grass, cow-grass, Dutch clover, and hop. The oats were very thick and long; in consequence of which the grass-plants in some places did not thrive, for in spots of five or six feet square there was no appearance of any. I had eight quarters of oats *per* acre. I fed the grass with sheep the years 1796, 1797, and till August 1798, when I again ploughed it, and dragged in some vetches. I had a great crop; and, in the spring

1799 fed it off with sheep, folding them on the ground, as they eat the vetches. When this was done, I made a fallow, as good as a wet summer would admit of, and sowed it to wheat again, at the usual season, without any additional manure, and had about twelve bushels *per* acre, a good crop for this year, some lands in this neighbourhood not producing more than six or seven. The acre in this country is customary measure, not statute.

Finding this land drained so well, I have every winter drained a little, as fast as I could get the land cleansed, and last winter I completed the whole ninety-three acres. I have drained a great many acres of land besides these, and have had it done in the same manner, which answers extremely well. Twenty acres of the above ninety-three I have kept in pasture, having in the space of five or six years dressed it over twice, with about fifteen tons *per* acre each time. My manure consists of dung, earth, soap-ashes, and the scrapings of the turnpike roads: this latter article answers remarkably well on a strong clay soil. The whole of these ninety-three acres are on such a soil, with some gravel. During the summer it is necessary to turn it twice, in order that it may be well mixed; and by doing this, it is brought to a fine mould; when spread on the lands, once brushing over with some thorns makes it soon disappear. The above twenty acres are at this time worth 25*s.* *per* acre. In November, 1795, I began ploughing another piece of ground of fourteen acres. From the manner in which this piece of ground lay, my servant could not throw it plain, (it being left, the last time it was ploughed, in six-furrowed ridges,) so as to bring it with any advantage to a crop in the spring. In April, 1796, I ploughed it across; after this worked it well with drags and harrows. Finding this would not do, owing to the turf not being sufficiently rotten, which I account

account for by lying in a rough open state all the winter,) I set some men to hack it over; and harrowing it well with four horses abreast, it became tolerably fine, with the exception only of the rush and sedge, of which there was a great abundance. The weather being dry, I employed twelve or fifteen women and boys beating over these rush and sedge roots, in order to get them out of the earth, some men going after them with three-pronged forks, throwing it in heaps, and burning it. By doing this, I raised a great number of ashes, which I spread over the land, and, as soon as sufficiently cold, I harrowed in some turnip-seed, which came up very well; but the land being of a close stiff nature, they did not get larger than about the size of a cricket-ball; I had them hoed, otherwise they would not, in my opinion, have grown to that size. I kept 450 sheep on them, with a little hay, a month and a few days. In the month of May, previous to this, I put on the headlands 140 hogsheads of lime, which I caused to be well turned and mixed, and as the sheep ate the turnips, this was carried and spread on the land. The latter end of October it was sown with wheat, and produced a good crop, averaging better than twenty bushels *per* acre. As soon as the wheat was carried off, it was ploughed, and sowed to vetches, (a greater burden than lands of the value of 40*s.* *per* acre could produce,) and in the spring 1798 fed them off with sheep, folding them by night where they fed by day. I always make it a point, as soon as the sheep have cleared a day's work for the plough, to plough the land; by doing this I preserve the manure of the sheep from the sun, and turn in what vetches were left, which, in my opinion, is equal, if not superior, to the droppings of the sheep. I have observed, that where the greatest quantity has been left and ploughed in, that part of the ground gene-

rally

rally works much lighter at seed time, and that at harvest the wheat is superior. This may not do so well on a light sandy loam. I finished sowing this field to wheat (the second time) by Michaelmas, 1798. I was obliged in May to cut off all the tops of it, (as I did in the other field,) in order to keep it standing. When harvest came, I had sixty tithing *per* acre, which produced no more than 300 bushels and about two pecks, in the whole field. If the kern had been such as it was the year before, I should, I believe, have had thirty bushels *per* acre. Last year it was sown to barley and grass-seeds. As the barley is not thrashed, I cannot state exactly the quantity grown, but from appearance it was judged to be about twenty bushels *per* acre; a good crop for this country last season: the grass-plants looked remarkably well, and consist of the following sorts: rye-grass one peck, cow-grass six pounds, Dutch clover two pounds, cock-grass two pounds; this I allowed for every acre. It is customary in this country for the tenant to be at the expense of the grass-seeds. The hop-grass did not answer in the other field; it is my opinion, that the soil is too heavy and close for it. I very much disapprove of mowing the first year after laying down, particularly on strong lands.

A gentleman living only a mile from me, three years since, laid down a piece of land entirely with cow-grass, the soil very similar to mine. Having no sheep to feed it with, he mowed it twice the first year. By doing this, the grass plants, &c. were completely killed. It is my opinion that the feeding off young grass with sheep will improve it, as well as improve the land, particularly rye-grass; for if this is once suffered to get long and dry, no cattle will feed on it. Cow-grass and Dutch clover appear to me to flourish most on strong lands. This, and every

every other sort of grass seed, I should recommend to be sown with flax, barley, or black oats, and for these two reasons: flax is in general sown from the middle of April to the early part of May, and drawn the early part of July, the roots of the flax being so small the grass-plants are not injured by drawing it; on the contrary, it moves and adds fresh earth to the plant. If the land is good for barley, I should prefer it to oats, because in general oats are sown thicker than barley, and consequently the sun and air are more excluded from the grass-plant. If oats must be sown, I should sow the small black oat, the straw being so much finer in the stalk, and much shorter. The sun and air having more power, the grass-plants of course get much stronger.

In November, 1796, I ploughed another field of eleven acres, and threw it very plain. As soon as it was finished ploughing, I had it rolled with a heavy roller, that it might lie as close as possible all the winter, in order to rot the spine or turf the better. In the early part of April I dragged in some black oats; shortly after they were up, an easterly wind (to which this field lay quite exposed) struck them very yellow, exactly like straw, and the ground being so very poor, they never recovered it. The ground lay in this state till September, when it was ploughed across, and so it remained till the March following. The frost having opened it, and, by dragging and harrowing it well, I brought it to be tolerably fine. In May I dressed it over with twenty hogsheads of lime *per* acre, (which was well mixed with the headlands in April,) then ploughed it as thin as possible, and sowed some turnip-seed, which came up very well, but did not flourish, owing, in my opinion, as I have before observed, to the soil being too stiff and heavy for turnips. At Michaelmas, 1798, it was sown to wheat, without

out any other manure. The crop produced me nineteen bushels *per* acre. As soon as the wheat was carried off the land, I sowed it to vetches, (which I am convinced, from experience, is the best artificial sowing on strong lands,) and I had a very good crop, which I fed off with sheep in the same manner as before described, and last Michaelmas it was sown the second time to wheat, without any additional manure; at present it is impossible for plants to look better. I intend taking three crops from this field, as I did from the last, and then to lay it down for three years, I think no land should have more than three crops of corn without rest; at the same time sowing between those crops some sort of artificials for sheep-feed, which will keep the land clean, and in good condition. The soil will dictate to the farmer what sort of artificials to sow for his advantage. I have at this time another field of ten acres in fallow, which has been ploughed these twelve months. I intend pursuing exactly the same method with this and the remaining thirty acres as I have done with the former; and, when finished, I have no doubt but that the ninety-three acres will be worth one hundred pounds *per annum.*

I trust it will appear, by the experiments which I have made, how practicable it is to improve a great part of the waste lands of this kingdom. The expenses at first sight appear great, but the industrious farmer would very soon find his advantage by laying out a few pounds in the same manner as I have before described. I have been more particular in pointing out the different crops that I have had than perhaps is absolutely necessary, but my reason for doing this is, that the Honourable Board may the better be enabled to judge of the expense and of the profit; likewise the great advantage that may follow to the community at large by converting *such lands* to good pasture and tillage.

Method

Method of connecting Iron Bars, and coating them with Lead, so as to form solid Pillars for Light-Houses, on Rocks covered at high Water, without being subject to Corrosion from the Action of Sea-Water.

By Captain JOSEPH BRODIE, *of the Royal Navy.*

With an Engraving.

From the TRANSACTIONS of the SOCIETY for the Encouragement of ARTS, MANUFACTURES, and COMMERCE.

The Gold Medal was voted to Captain BRODIE *for his sundry Marine Improvements, particularly for this Invention. A Model is deposited in the Society's Repository, and also of a Cone or Break-water, of Cast-Iron, invented by him.*

Fig. 1, (Plate II.) A, shews four rods of cast-iron, composed of a number of pieces two feet long, rivetted together, in a manner explained by the plate, so as to produce the effect of one bar of the thickness of the whole. B, a tube of cast-iron, formed from a number of separate pieces, each about ten inches long, and which, when placed round the iron rods above mentioned, and then screwed together, form a mould, into which the melted lead is to be poured, to coat the iron-rods. C, a portion of the rods covered with the melted lead, so as to form a cylindrical pillar apparently of lead, the iron being perfectly coated therewith.

Fig. 2, D, shews the manner in which the hollow cylinder is formed to any length required, by the junction of a number of semi-cylinders rivetted together, and fitting each other. E, the side flanges screwed close toge-

ther. F, the end flanges also screwed together as prepared for the melted lead.

After a certain portion of the iron rods are coated with lead, the lower parts of the tube are taken off and placed higher up; by which repeated changes, a few tubes will answer the purpose to coat any length of the iron rods.

Description of an improved Mill for grinding Painters' Colours, invented by Mr. JAMES RAWLINSON, *of Derby; also of one for grinding Indigo in a dry State, used at Manchester by Mr.* CHARLES TAYLOR.

With an Engraving.

From the TRANSACTIONS of the SOCIETY for the Encouragement of ARTS, MANUFACTURES, and COMMERCE.

The Silver Medal and Ten Guineas were voted to the Inventor, and a Model is placed in the Society's Repository.

THE hitherto very unmechanical, inconvenient, and highly injurious method of grinding poisonous and noxious colours, led me first to imagine a better might easily be contrived for that purpose. It must be obvious to every person, that the method hitherto adopted of grinding colours on an horizontal marble slab, with a small pebble muller, requires the body of the person who grinds to bend over that slab, and consequently his head; which causes him constantly to inhale the noxious and poisonous volatile parts of the paint, which is not unfrequently ground with oil saturated with litharge of lead; and if we may judge from the very unhealthy appearance of these men, accustomed to much colour-grinding, it

it should seem the bad effects of this employment require a speedy remedy.

A machine, of which I now send the Society a model, has not only the advantage of being an effectual remedy of this extensive and severe evil to recommend it, but it grinds the colour much easier, much finer, and much quicker, than any method hitherto adopted. Having occasion for a considerable quantity of colour-grinding in the profession in which I am engaged, and that in the finest state possible, and having made use of this machine for several years, and being more and more convinced of its utility, I thought it my duty to present it to the Society of Arts, hoping that it might not be altogether unworthy of their attention. The roller of the machine that I use is sixteen inches and a half in diameter, and four inches and a half in breadth. The concave muller that it works against covers one-third of that roller: it is therefore evident, that with this machine I have seventy-two square inches of the concave marble muller in constant work on the paint, and that I can bring the paint much oftener under this muller in a given space of time than I could by the usual method with the pebble muller, which is seldom more than four inches in diameter, and consequently has scarcely sixteen square inches at work on the paint, when my concave muller has seventy-two. I do not mean to say that a roller, the size of that which I now use, is the largest which might be employed; for, truly, I believe that a roller two feet in diameter, with a concave muller in proportion, would not be hard work for a man; and then the advantage to the public would be still farther increased.

This machine will be found equally useful for the colours ground in water as for those ground in oils; and I

doubt not but the great importance of this simple machine will be very soon generally experienced in all manufactories where colours are used. The labour necessary with this machine, in grinding colours exceedingly fine, is very easy. It is useless to enter into any minute description in this place, as a bare inspection of the machine must sufficiently explain itself.

To the colour-man it would evidently be an essential saving of labour, and consequently of expense, which will probably have some weight as a recommendation; and the advantages to the colour-grinder have been already stated.

I have made a rough sketch of the machine, with letters of reference, as supposing this may better explain the process.

Fig. 3, (Plate II.) A, is the roller or cylinder, made of any kind of marble; black marble is esteemed the best, because it is the hardest, and takes the best polish. B is the concave muller, covering one-third of the roller, and of the same kind of marble, and is fixed in a wooden frame *b*, which is hung to the frame E at *ii*. C is a piece of iron, about an inch broad, to keep the muller steady, and is fixed to the frame with a joint at *f*. The small binding screw, with the fly-nut, that passes through the centre of the iron-plate at *c*, is for the purpose of laying more pressure on the muller, if required, as well as to keep it steady. D is a taker-off, made of a clock-spring, about half an inch broad, and fixed similar to a frame-saw in an iron frame K, in an inclined position to the roller, and turning on pivots at *d d*. G is a slide-board, to draw out occasionally, to clean, &c. if any particles of paint should fall from the roller, and which also forms itself for the plate H, to catch the colour on

as

as it falls from the taker-off. F is a drawer, for the purpose of containing curriers-shavings, which are the best things for cleaning paint-mills. E is the frame.

Previous to the colour being applied to the mill, I should recommend it to be finely pulverized in a mortar, covered in the manner of the chemists when they levigate poisonous drugs *. This process of dry-grinding is equally necessary for the marble slab now in use; after which it should be mixed with oil or water, and with a spatula or pallet-knife put on the roller, near to the top of the concave muller, and the roller turned round, which takes the colour under the muller without any difficulty, and very few turns of the roller spread it equally over its surface. When it is perceived sufficiently fine for the purpose required, it is very easily taken off by means of the taker-off described, which must be held against the roller, and the roller turned the reverse way, which cleans it very quick and very completely; and the muller will only require to be cleaned when you desist or change the colour. It is then turned back, being hung on pinions to the frame at *i i*, and cleaned with a pallet-knife or spatula very conveniently. Afterwards a handful of curriers-shavings held on the roller, with two or three revolutions, cleans it effectually; and there is less waste with this machine than with any marble slab.

As to the quantity ground at once on this mill, it must be regulated by the state of fineness to which it is required to be ground. If it is wanted to be very fine, a smaller quantity must be put on the roller at a time; and

* Or rather in an improved mill, used at Manchester by Mr. Charles Taylor, for grinding indigo in a dry state, of which I have annexed a drawing, and reference, to render the whole business of colour-grinding complete.

as to time requisite for grinding a given quantity of colour, this will also depend on the state of fineness to which it is ground. I have observed, that my colour-grinder has ground the quantity of colour which used to serve him *per* day, with this machine, in three hours, and, as he said, with ease. The colour also was much more to my satisfaction than in the former way, and attended with less waste.

I have mentioned the pulverizing the colours in a covered mortar, which would prevent waste, and prevent the dust and finest parts of noxious colours from being injurious to the grinder. In some manufactories, where large quantities of colours, prepared from lead, copper, and arsenic, are used, this precaution is particularly necessary. I do not mean to say that my machine is intended to supersede the paint-mill now in use for coarse common colours. It is intended for no such purpose; but to supersede the use of the very awkward and unmechanical marble slab now in use, and on which all the colours for china manufactories, coach-painters, japanners, and colour-manufacturers for artists, &c. &c. are now ground.

Several of the colour-manufacturers have expressed to me their great want of such a machine; and that I had no desire of troubling the public with a machine that would not answer, is evident, from my having used it several years before I presumed to recommend it to their attention. Being, therefore, now completely convinced of its utility, and hoping that it might relieve a number of my fellow-creatures from a dangerous employment, I have ventured to commit it to the protection of the Society of Arts; hoping, through their means, to see its ultimate success. And, farther, to give the Society the most

most complete assurance in my power, I have annexed the opinion of a very ingenious and mechanical friend of mine, who has frequently seen it work.

When the colour is ground, I recommend the following mode of tying it up in bladders, in preference to the usual method. Instead of drawing the neck of the bladder close, in the act of tying it, insert a slender cylindrical stick, and bind the bladder close around it. This, when dry, will form a tube or pipe, through which, when the stick is withdrawn, the colour may be squeezed as wanted, and the neck again closed by replacing the stick. This is not only a neater and much more cleanly mode than the usual one of perforating the bladder, and stopping the hole with a nail, or more commonly leaving it open, to the prejudice of the colour, but the bladder, being uninjured, may be used repeatedly for fresh quantities of colour.

N. B. The barrel of a quill may be tied, in place of the stick, into the neck of the bladder, with its closed-end outwards, which will keep the colour secure in travelling, and when used, the end of the quill being cut off, it may afterwards be closed by a stick.

A certificate from Mr. Thomas Swanwick, of Derby, and also from Mr. John Middleton, of St. Martin's-lane, confirming the above statement, accompanied these papers.

REFERENCE to the improved MILL for grinding INDIGO, or other dry Colours.

Fig. 4, (Plate II.) L, represents a mortar, made of marble or hard stone; one made in the common way will answer. M, a muller or grinder, nearly in the form of

of a pear; in the upper part of which an iron axis is firmly fixed; which axis, at the parts N N, turns in grooves or slits, cut in two pieces of oak, projecting horizontally from a wall, and when the axis is at work are secured in the grooves by iron pins O O. P, the handle, which forms a part of the axis, and by which the grinder is worked. Q, the wall in which the oak pieces N N are fixed. R, a weight, which may occasionally be added, if more power is wanted.

Fig. 5, shews the muller or grinder, with its axis separate from the other machinery; its bottom should be made to fit the mortar. S is a groove cut through the stone.

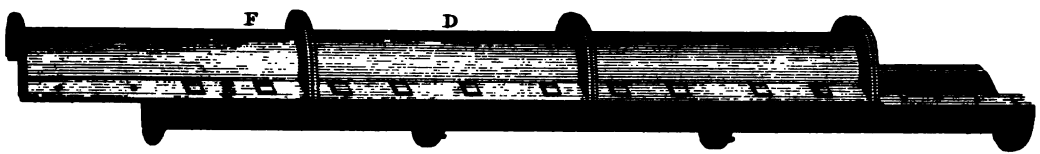
On grinding indigo, or such substance, in a dry state, in this mill, the muller being placed in the mortar, and secured in the oak pieces by the pins, the indigo to be ground is thrown above the muller into the mortar; on turning the handle of the axis, the indigo, in lumps, falls into the groove cut through the muller, and is thence drawn under the action of the muller, and propelled to its outer edge within the mortar, whence the coarser particles again fall into the groove of the muller, and are again ground under it; which operation is continued till the whole of it is ground to an impalpable powder; the muller is then easily removed, and the colour taken out.

A wood cover, in two halves, with a hole for the axis, is usually placed upon the mortar, during the operation, to prevent any loss to the colour, or bad effect to the operator.

Memoir

Fig. 1.

Fig. 2.



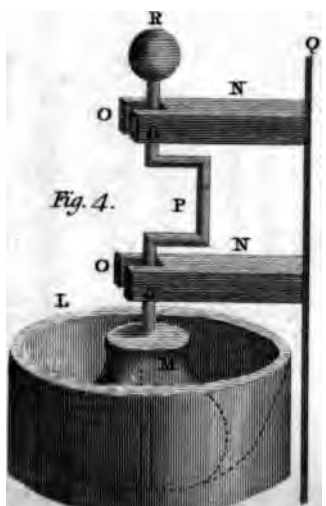
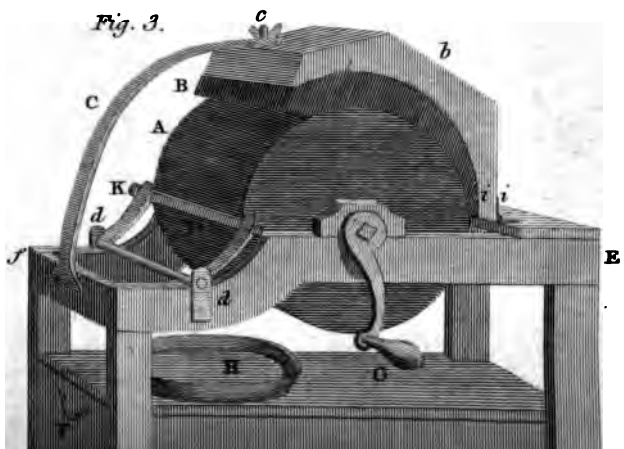


Fig. 4.

Fig. 5

Fig. 3.



Memoir on Verdegris. *By M.* PROUST.

From the JOURNAL DE PHYSIQUE.

COPPER, oxygene, acetic acid, water—these are the ingredients of that product of the arts which we term *verdegris*. But what combinations do these ingredients form with each other? or rather, what is really verdegris? This is a question that has indeed been often examined; but, in my opinion, not yet as much as it deserves. I shall therefore endeavour to add something to the investigations of my predecessors.

If we treat a hundred parts of verdegris with potash, we obtain from them (from that which I have before me) 46 of copper, oxydated at 25, to the hundred; the rest is water, and the acid which unites with the potash. Now if the water, the acetic acid, and the oxyd, are really the principles of the verdegris, it is easy to see that these principles can form with each other only some of these three well-known combinations, *viz.*

The acetate at the maximum of acid.
The acetate at the minimum of acid.
The hydrate of copper.

Let us take a summary view of the principal properties of these combinations, and then see if they will aid us to discover those which constitute verdegris.

Acetate at the Maximum.

This acetate is a soluble crystallizable salt, which cannot be decomposed by carbonic acid. The acetate may be green or white; that is to say, with a base of hydrate or a base of oxyd, and consequently hydracetate, or anhydrous acetate.

The anhydrous acetate is an accidental product: it seems to me that it presents itself during the distillation of verdet, performed for the purpose of obtaining from it what is termed radical vinegar. When the acetate has begun to feel the impression of the heat, and to be deprived of its water, the white acetate rises in flowers to the roof and into the neck of the retort. Every one may remark its formation, as well as the rapidity with which it is re-dissolved and passes again into the green state, as soon as it encounters a puff of humid vapours. I have, nevertheless, more direct proofs respecting the existence of this salt than that which I have just adduced; I therefore greatly wish that some one might occupy himself with it, in order to give greater precision to our ideas respecting the nature of this acetate.

Acetate of the Minimum.

This is a salt of a blueish-green colour, perfectly insoluble, and upon which boiling water has not the smallest action. Carbonic acid, suitably applied, does not decompose it, nor even separate from it any part of its base: this proves that the acetic acid, though in a proportion much inferior to that which constitutes the soluble acetate, adheres nevertheless to all parts of the oxyd, secures their state of combination, and leaves none of them naked. Thus, as there are sulphates, nitrates, muriates of copper at the minimum of acid, we have also an acetate of copper of this kind.

This second acetate is obtained by throwing black oxyd or hydrate into a solution of the first. The soluble acetate in this case undergoes the same changes as those of which we have given account in speaking of the nitrates, the sulphates, and the muriates kept upon their oxyd.

The

The sulphuric acid separates the acetic acid from this second combination as it does from the first. Distillation decomposes it. Much water passes over; a little vinegar escapes; the rest is destroyed; and then its radicals contribute to the disoxydation of the copper, which is found at the bottom of the retort. For want of vinegar, however, the disoxydation of its base is but partial, whereas it is complete after the distillation of the soluble acetate and the verdegris. Aqueous sulphuric acid, applied to these residues, dissolves the oxydated portion, and leaves the metal naked: this is one means of analyzing them.

Hydrate.

The hydrate of copper is a combination of oxyd and of water in uniform proportions. It is the base of all the coloured salts of copper. When the alkalies are applied to it cold it abandons the acids, and is thrown down of its proper colour. If the alkalies are boiling, the oxyd losing the acid, and the water which constitute it hydrate, is reduced to the black oxyd.

All the acids cause the hydrate to pass into the saline state. If, for example, carbonic acid is applied to it, it loses its colour and volume. It is changed into a powder of a beautiful green colour, soluble in the acids with effervescence, and which differs from the hydrate by the circumstance, that boiling water has no action upon it; that is to say, the hydrate becomes carbonate, as it is changed into sulphate, nitrate, &c. when it is encountered by the sulphuric, nitric, or other acid.

The acids in dissolving the hydrate do not exclude the water from it, neither do they exclude it from the hydrates of potash, of soda, of barytes, &c. The water remains in the combination. This may be hydrated or anhydrous, according to its nature. If it remains in it,

it shares the condensation of the other elements; and like them, finally, the water maintains itself in it in a constant proportion.

The fixed or volatile alkalies do not dissolve the oxyd of copper, except in as far as it is hydrated, or as it can assume that state, in proportion as they solicit it to the union. The water combined with the oxyds frequently causes their colours to disappear, giving them a new one, which generally is more brilliant. Finally, the hydrates are not modified as to their colours by the acids which have co-operated in their formation, which would not fail to be the case if they constituted part of them.

Let us now proceed to the verdegris. When we dilute it with a large quantity of water, we see it separate into two substances, the one soluble, of a green colour, the other insoluble, and of a blue colour. This latter has not the pulverulent tone, the ordinary weight of the sulphates, the nitrates, the muriates, the acetates at the minimum of acid. It is crystalline, scaly; it thickens the solution of the acetate, and appears to adhere to it more than it would adhere to pure water; accordingly its filtration is very difficult, but, when once effected, the solution presents nothing more than ordinary acetate.

The following are now the three grand characteristics by which the blue precipitate of verdegris distinguishes itself from the acetate at the minimum.

1. This precipitate kept under water is gradually decomposed, and reduced into black oxyd.

2. Thrown into boiling water, it immediately loses its colour, and is reduced into black oxyd.

3. If we cause to pass into a solution of verdegris a sufficiently continued current of carbonic gas, and take care to shake the flask from time to time, in order to facilitate the solution, the verdegris is analyzed. The liquor

quor clears; the deposit loses its colour, sinks from the volume of five or six inches to that of a few lines, and presents at last only a mixture of acetite and carbonate, which filtration separates with the greatest facility.

May we not now conclude, from these results, that the blue deposit, which separates from the verdegris in the proportion of 40, 45, 50, according to its quality, cannot be an acetate at the minimum, as I had imagined at first? In fact, it is not in the nature of this acetate to be decomposed by boiling water, nor by carbonic acid; and, finally, we see that this deposit, this precipitate, has no other properties than that which we extract from the solutions of copper by the saline or earthy alkalies, and which I have designated by the denomination of hydrate.

Now we discover why boiling water reduces verdegris into black oxyd and crystallizable acetate. Now we see why, in the preparation of verdegris, it is indifferent whether the plates are sprinkled with water or with vinegar. Finally, we see to what we are to attribute the defect complained of in verdegris when employed in painting; namely, of changing to a green colour, which probably proceeds from the circumstance that the hydrate attracts to itself the carbonic acid of the atmosphere, as happens with the other metallic, earthy, and alkaline hydrates.

It may be proper here to call the attention of painters to the carbonate of copper, the colour of which cannot be liable to the same deterioration as that of verdegris. This carbonate is of a beautiful green colour. It has nothing of a saline nature like verdet or the soluble acetate, and it might be procured cheap if it were extracted from the cupreous parting waters. In order to obtain it in its greatest beauty, and the least possible bulk, the solutions

tions of copper must be poured into the boiling alkaline ley, washed, dried, and preserved. The success of this colour is not liable to vary in the operation like that of the blue cinder.

A hundred parts of verdegris, decomposed by water alone, yield 43 of crystallized acetate and 27 of black oxyd, in all 70.

When this decomposition is performed in close vessels, neither acid nor gas escapes; the verdegris is therefore formed of

Acetate	43
Black oxyd	27
Water	30

If we distil a hundred parts of the same verdegris, we obtain from it a considerable quantity of liquid, which amounts to 46, the residue is 40. The gases which escape in this distillation are also very voluminous, for to 100 grains we obtain 150 or 155 cubic inches, of which about 135 are carbonic acid, and the rest, or about 20, are an inflammable gas, which has appeared to me by the colour of its flame to be a mixture of carbonated hydrogene and oxyd of carbon.

The forty-hundredths of residue are a mixture of metal and of carbon. If weak nitric acid is poured upon them it separates about three parts of the latter, which reduces the metal to 37. This result corresponds within a fraction to the 46 parts of oxyd which verdegris yields when it is decomposed with potash. If, from the 46 hundredths of aqueous products, we deduct 30 for the water which does not belong to the acetic acid, we find that the latter amounts only to 16 hundredths; nevertheless the verdegris contains this acid in a much larger proportion, but the heat has destroyed a great part of it, and

it

it is the remains of it that have contributed to the disoxydation of the copper.

Verdegris contains likewise a much more considerable quantity of water than belongs to the acetate and the hydrate of which it is composed; for if 72 parts of oxyd take only 28 of water to constitute them hydrate, the 28 which we find in the quintal of verdegris ought to require not more than five or six. Thus 46 parts of crystallized acetate, and from 29 to 30 of hydrate, making together 77, it is evident that 23 of water must be added to these 77, in order to complete the quintal. Verdegris is therefore, in this point of view, a sort of combination which has the property of consolidating a very considerable portion of water. Nor let it be imagined, that that which served as the subject of these researches was verdegris recently prepared, for I had had it in my laboratory 18 years. But this composition will not be the only one which possesses the property of thus consolidating more water than strictly belongs to its essence. Soap is in the same predicament. A quintal of oil may furnish from 140 to 300 pounds of soap, without ceasing to assume a solidity capable of imposing upon the eye of those who are not acquainted with this kind of fraud. And in all the proportions that may be comprehended between these two extremes, the soapy combination always loses, at the moment of coagulating, a portion of heat which raises the temperature from 15 to 25. This evidently proves that the water consolidates itself between its parts, as it does between those of the most aqueous crystals.

Verdegris does not contain carbonate of copper, although it be formed in circumstances nearly similar to those in which the oxyd of lead absorbs so large a quantity. This is shewn by the following experiment. If

any

any acid is poured upon wet verdegris, it dissolves in it without shewing the least sign of effervescence. The hydrate extracted from verdegris takes a very long time to wash it upon the filtre: during this time it attracts, like all compounds of that sort, a small quantity of carbonic acid from the atmosphere, which enables it to exhibit signs of effervescence when an acid is added to it; but it is easy to see that it is no longer such as it was in the verdegris; that is to say, it has begun to change its state.

The hydrate of verdegris kept in the liquor itself with which it has been diluted, and consequently in a solution of acetate, gradually decomposes the latter by carrying off that portion of acid which constitutes its difference from the acetate at the minimum. It insensibly reduces it to the condition of the latter, the liquor then loses its colour, approaches to the state of pure water, and the deposit, the tinge of which changes to a grey, is now only a mixture of black oxyd, of hydrate, and of acetate at the minimum.

Inferences.

In the formation of verdegris, the copper becoming oxydated, furnishes to this composition a base which does not differ from that which enters into the nitrates, the muriates, the sulphates, the carbonates, native or artificial.

This oxyd, in proportion as it forms, assumes the character of hydrate. In this state one part saturates the vinegar which it meets with, while the other remains in the same state for want of vinegar.

Those kinds of verdegris to which this acid is abundantly supplied, are almost all changed into soluble acetate; such is, according to the observations of Chaptal, that which is prepared in Dauphiny.

If

If the change of copper into verdegris is continued by merely wetting the plates with water, it is probable that, among the efficient causes of this conversion, the affinity of the oxyd for water is to be reckoned as one of the principal.

The verdegrisation of copper would not, it seems, take place if it were not commenced by an acid; but this subject still requires investigation, for it would perhaps be possible to set it in action with a mineral acid instead of vinegar, which might lead to useful results.

Cuprate of Ammoniac.

Copper, which we have seen form with the marine acid two muriates relative to its two oxydations, may also form with ammoniac two combinations of the same order. One of them has long been known, namely, the blue cuprate; the other could not be so, because the second oxydation of the copper, which was to lead us to the knowledge of it, was unknown.

Fill a flask with ammoniac, highly coloured by the oxyd of copper; add to it a portion of copper in powder, that, for example, which remains after the distillation of verdegris, and then let the flask be carried in the pocket, in order to give it motion.

At the end of some days the colour is found to have entirely disappeared, the liquid has the appearance of water; but if a portion of it is gently poured into a glass, it rapidly becomes coloured by the absorption of the atmospheric oxygene, and all the phenomena which the solution of the white muriate in ammoniac produces are renewed.

As for the theory, it will already be surmised by the reader. The oxyd at 25 *per cent.* colours the ammoniac

blue, but the oxyd at 16 has not this property. The copper therefore takes from the first the portion of oxygene which constitutes the difference between the oxydation at twenty-five and sixteen, and, reducing the first from the maximum to the minimum, it constitutes itself oxyd of the second species. At present, therefore, every thing seems to prove that copper is, like most other metals with which we are acquainted, susceptible of two states of oxydation.

Description of an Apparatus for distinguishing Gold Coin of the legal Standard from that of baser Alloy; adapted for general Use.

With a Plate.

From the ANNALES DES ARTS.

WE meet with many pieces of gold in circulation which are clipped or altered in their weight; but as these are not taken in payments without weighing, it is easy to guard against any loss by them. The case is different with respect to the counterfeit Louis d'ors, so that an easy and certain method of distinguishing them is greatly to be desired. For this purpose it would be sufficient to render familiar the use of an instrument which exists already, under the name of the Hydrostatic Balance, in all collections of philosophical instruments, and by which one may easily distinguish (though with the same weight in the ordinary balance) the pieces of the just standard from those of base alloy; that is to say, such as do not contain the quantity of gold prescribed by the law.

Some time ago M. Vincent contrived an instrument particularly destined for this purpose, which he termed

pèse-

pèse-monnoie (money-weigher), and which was approved of by the National Institute upon the report of its committee. Hence one should have imagined that this instrument would soon have come into universal use, at least among that class of people who, from having a great deal of specie passing through their hands, are in continual danger of having base money imposed upon them. That this has not proved the case, can perhaps be attributed only to the very perfection which this artist had endeavoured to give to his instrument, which he made of silver, to the properties which he attempted to combine in it, and to the high finish of the workmanship, circumstances which necessarily enhanced its price, and seemed at the same time to require an experienced hand to use it with advantage. It was therefore perceived that, in order to effect the desired purpose, a less costly instrument ought to be contrived, which, instead of ascertaining the exact standard of a piece of coin, should serve solely to determine whether it is or is not of the just alloy, and which besides should not require either more knowledge, dexterity, or attention, than the ordinary balance. That this has been accomplished will appear from the following description of the instrument and instructions respecting the manner of using it.

Explanation of Plate III.

The instrument consists of a hollow ball of copper, twenty or twenty-one lines in diameter, having below a ring or fork, and surmounted with a stalk, upon which divisions are marked, the whole surmounted with a small flat capsule.

Fig. 1 represents the instrument in half size.

Fig. 2 represents it in another direction, charged with the piece which is called the standard, and floating in a vessel filled with water.

The standard, which is a piece of copper, of a determinate weight, that shall produce in the liquid the same effect as a piece of gold of forty-eight livres, or two pieces of twenty-four livres, is represented separately, Fig. 3.

Fig. 4 represents the piece which must be placed in the ring of the instrument at the side of the single Louis-d'or, or twenty-four livre piece, which is to be tried.

Lastly, we must have a small box, containing divisions of weights in grains; and this is all that is required.

Manner of employing the Instrument. Fill the glass with water nearly to the height indicated by the dotted line A, Fig. 1, in order that the instrument being immersed in it, the water may rise near enough to the margin to enable us conveniently to discern the point of the stalk which corresponds with the level of the liquid.

The water having been poured into the vessel, immerse the instrument in it, taking care previously to dry it: place the standard in the ring, and the whole is ready for use.

Verification of forty-eight Livre Pieces, or double Louis. To know whether a piece of gold, a double Louis for example, is of the just standard, examine first the point of the stalk which at the present moment is at the level of the water, whether it is half a degree above or below Zero: there will hardly be more than this difference.

This being done, take the standard out of the ring, and put in its place the forty-eight livre piece. The instrument being now immersed again in the water, if it descends

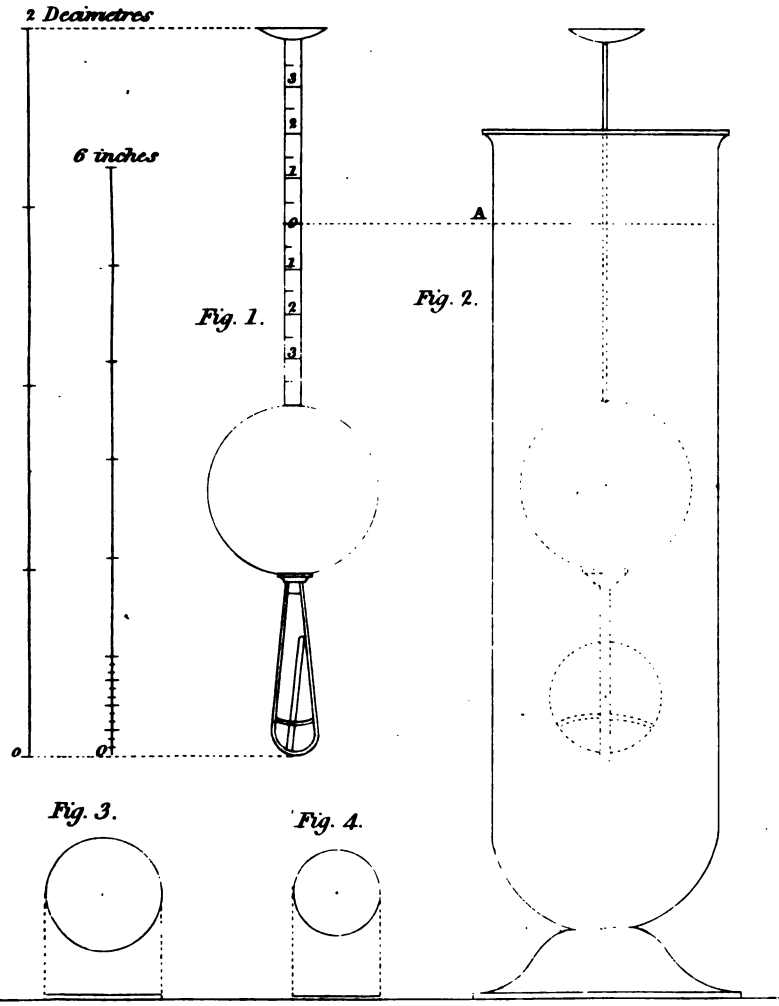


Fig. 1.

Fig. 2.

Fig. 3.

Fig. 4.

descends to the same point that has been observed, the piece is tried; it is of the proper standard.

If the instrument does not descend to the same point the piece is either of base alloy, or deficient in weight. It is then put into a common balance, and at the side of it are added the number of grains that are required to bring it into equilibrium with the 288, which the double Louis ought to weigh. The piece is then inserted anew into the instrument, and upon the capsule at the top are placed as many grains as it was found to be deficient in weight; if it descends to the point observed at first with the standard it is good; if it remains more elevated above the surface of the water, were it only by half a division, the piece is of base alloy.

One might also weigh the piece first, and putting it into the ring of the instrument, add afterwards the grains by which it might be deficient.

Verification of the twenty-four Livre Pieces, or single Louis. The operation is the same for the single as for the double Louis, with only this difference, that we place at the same time in the ring of the instrument the small piece of copper, Fig. 4, which is destined to give with it the same point of equilibrium as the double Louis or the standard.

We must always first of all observe the point on the stalk of the instrument which corresponds at the present moment with the surface of the water, and which indicates that at which the instrument must again be found when the Louis and the supplementary piece have been placed in the ring instead of the standard.

If the Louis has been clipped, or part of its weight worn away, we put, in the same manner as with the double Louis, the grains by which it is deficient into the capsule of the instrument; it must then descend to the same

same point which has been observed with the standard, otherwise the piece is of base alloy.

Any one may therefore secure himself in future, at little expense, and without any difficulty, against being imposed upon by any counterfeit gold coins, since he has the means of distinguishing with certainty such as are not of the legal standard, or, in other words, which do not contain the quantity of gold which determines their value.

New Observations on the Nature of Tannin.

By M. TROMSDORFF.

From NEUES ALGEMEINES JOURNAL DE CHEMIE.

IT has long been known that a very great number of vegetables communicate to water, in which they have been immersed, the property of throwing down a black precipitate from the combinations of iron with the acids; a property, which was supposed to reside in a peculiar principle, called the *astringent* principle, on account of the manner in which it affects the organ of taste. Several phenomena had already led the Dijon academicians to suppose that this principle partook of the nature of the acids; and the experiments of the immortal Scheele, who obtained from the gall-nut a real acid in a crystalline form, seemed to have given complete certainty to this opinion, so that *astringent principle* and *gallic acid* were considered as synonimous terms.

M. Seguin imagined latterly to have discovered a real difference between the gallic acid and the astringent principle, and he demonstrated, by experiment, that the latter actually possesses some properties which do not belong

long to the other. Finally, the observations of Proust, in support of this fact, leave no doubt remaining, although J. A. Scherer, of Vienna, has lately, though not upon sufficient grounds, asserted the contrary, and his assertion has found an advocate in A. N. Scherer, of Dorpat.

It is not my design to give here a history of the astringent principle, since the experiments of Seguin, Proust, &c. are recent, and no doubt universally known; besides which Karsten has lately given an account, in Scherer's Chemical Journal, vol. VII. p. 472, of the results hitherto obtained from the inquiries respecting the astringent principle.

Though it is at present completely demonstrated from all the experiments hitherto known, that the gallic acid and the astringent matter, or tannin, *(materia scytodephica,)* are essentially different from each other, although they are almost always found combined together, it must nevertheless be admitted that we are still far from possessing an accurate knowledge of the properties of the astringent principle, and even that several of the facts which have been advanced are contradictory to each other, and repugnant to analogy. Finally, what renders this subject still more complicated, is the observation lately made by Proust, that the tannin itself is susceptible of anomalous variations; that is to say, there are, properly speaking, several sorts of tannin.

It is only by accurate experiments that results of more general certainty can finally be obtained; and this is what has induced me to make those of which I am about to give an account.

Whatever uncertainty there may still prevail respecting pure tannin, it seems nevertheless that it is the gall-nut that has been supposed to contain it in the greatest purity,

purity, for, in fact, the infusion of gall-nut mixed with a solution of glue furnishes, in the most manifest and abundant manner, this remarkable product, which is insoluble in water, elastic when fresh, and incorruptible, and for this reason also I have chosen the gall-nut for my experiments.

Experiment I.

Three pounds of the best gall-nuts, reduced to a fine powder, were put with forty pounds of distilled water into a glass matrass, and carefully agitated during three days, in a temperature of 15° Reaum.; after which the whole was strained through a cloth. The residue was then put again into the matrass with the same quantity of distilled water, and treated in every respect as the first time. The same operation was repeated two more times. The last infusion was perfectly colourless, and had scarce any action upon a solution of iron. Cold water can therefore extract all the soluble parts of the gall-nut.

Experiment II.

All these infusions, which, mixed together, were of the colour of Rhenish wine, were evaporated, by a very cautious application of fire, in a porcellain vessel, upon the sand-bath, and reduced to one-fourth. When the liquor became turbid by cooling, it was passed through a cloth of a close texture. This loss of transparency proceeded probably from the extractive matter that was oxydated during the evaporation. After this, the whole was evaporated to the consistence of a thick juice, of a blackish brown colour, though sufficiently transparent; it was poured into several porcellain dishes which, after having been covered with paper, were placed upon a convenient apparatus, near to a heated stove. The whole was dry at the end of eight days, and could easily be detached from

from the plate in thin plates, by means of a sharp-edged spatule of glass. The whole mass, which was of a dark brown colour, weighed thirty ounces, nearly two-thirds of the weight of the gall-nuts: reduced into small pieces, and exposed again for several days upon a hot stove, it still lost an ounce and a half of its weight.

Experiment III.

The separation of the mass (Experiment II.) was performed at three times, by means of thrice its volume of alcohol prepared according to Richter's method, and tried by his alcoholimeter. In the last extraction it was scarcely possible to discover any remaining traces of gallic acid, of which the greatest quantity was found in the first; but I shall speak of its separation, and in another circumstance. In order to extract all the gallic acid, I still put, at twice, upon this residue, alcohol mixed with a tenth part of water, which gave a tincture of a dark colour, and, as cannot be doubted, charged also with a large quantity of tannin, which I rather chose to sacrifice, in order to disengage the whole of the gallic acid. Nevertheless, water, in which a little of the residue had been infused, still turned the tincture of turnsole red. The rest, when dried, did not weigh more than twenty-five ounces.

Experiment IV.

In order to ascertain whether the residuum (Experiment III.) still contained gallic acid, I put two ounces of it into a retort over a lamp, and gently heated them. No gallic acid was disengaged from them. For a counter-proof an ounce of gall-nut was heated with distilled water, and the dry residuum, obtained by the evaporation of the liquid, having been exposed to the same degree of heat, immediately yielded crystallized gallic acid, which at-

tached itself to the roof of the retort. But whence the acid re-action that took place in Experiment III.? Has tannin the property of reddening the blue tincture of turnsole, or is there still a free acid in tannin? I shall not venture to decide upon this question, though I rather incline to the latter opinion, knowing how small a quantity of acid is required to change the delicate colour of the turnsole paper. This quantity is so small, that it is perhaps impossible for chemistry to effect its separation.

Experiment V.

But, as I still suspected, in this tannin treated with alcohol, the existence of heterogeneous principles, either mucilaginous, extractive, or others, I attempted their separation also. As to the extractive part, I thought I could not do better, in order to obtain it, than to filtrate and evaporate a reiterated solution in distilled water; a tedious operation, which I repeated four times. At last no farther separation took place, and the dried mass dissolved entirely, and with transparency in water.

Experiment VI.

What led me to suspect the presence of mucilage in the tannin treated as described above, was the following experiment. I had dissolved an ounce of it in eight ounces of water, which I exposed, in an open vessel, in the middle of a warm room. A pretty thick mouldy pellicle was soon formed upon its surface. After having filtrated and heated the liquid, I placed it again in the same exposure, and no more of the mouldy substance was formed. I then compared this solution with another which I had just prepared, and I remarked no difference between them as to taste, smell, or their manner of comporting themselves with regard to glue,

the

the alkalies, &c. For this reason I thought it expedient to purify the whole of the tannin in the same manner. I therefore dissolved all my tannin in distilled water; I left the solution in a warm temperature, in glass vessels, slightly covered, till a thick mouldy pellicle was formed: I then filtrated and evaporated it to dryness. When this substance had been dissolved again, no more mouldy substance was produced. This process might perhaps be considered insufficient; but having found that there resulted from it no alteration in the tannin, which possessed all its principal properties as before, I concluded it to be very useful, since it is proper for extracting a heterogeneous substance which, according to all appearance, is of a mucilaginous nature. As to the astringent principle, it is evident, from the presence of its characteristic properties, that it has undergone no change in its nature: supposing that acetic acid had been formed, the heat would have volatilized it; besides, this acid, as the subsequent experiments will prove, has no particular influence upon tannin.

Experiment VII.

In this manner I imagined I had obtained pure tannin. I wished to make a trial of its manner of comporting itself with other substances, in order to compare it then with the tannin obtained according to Proust's different processes, and thus to arrive at determinate results. According to Proust, pure tannin is obtained by pouring a solution of carbonate of potash into a saturated infusion of galls, whence results a precipitate which, according to him, is real tannin; and nothing more is required, in order to obtain it pure, than to wash it with a little cold water. I therefore dissolved two ounces of my tannin in distilled water, and then poured into it a saturated solu-

tion of carbonate of potash till no more precipitate was formed. The result was a precipitate of a dirty yellow colour, and the supernatant liquid presented scarcely a tinge of pale yellow. I put the whole upon a filtre; the water which passed assumed a deep green colour after the filtration, and the whole filtre acquired likewise the same colour. This phenomenon proceeded probably from an absorption of oxygene, for a small quantity of acid of hydrothion * immediately deprived the green liquid of its colour. In order to preserve pure tannin upon the filtre, I poured, at six times, a small quantity of cold distilled water upon the residue, by means of which, indeed, an abundant solution was still produced. The residue was dried, marked with the letter A, and put by for the present in a well-closed glass vessel. The filtrated liquid was divided into two parts; the first of which was evaporated to dryness by a gentle heat, and its residue marked with the letter B. As to the other part, I subjected it to the following experiments.

1. A solution of fish-glue in distilled water did not render this liquid turbid, nor was any precipitate thrown down.

2. The liquid mixed with a small quantity of acid, no matter which, and afterwards with the solution of glue, immediately became clouded, and deposited the coriaceous mass with which we are acquainted.

3. This liquid being mixed first with the solution of glue, and afterwards with an acid, the result was the same.

4. On mixing the filtrated liquid, which was alkaline, and had no astringent taste, with an acid, and afterwards pouring upon it some drops of red muriate of iron, a precipitate of a blackish blue was thrown down.

* The author substitutes this term for hydrosulphuret.

Experiment

Experiment VIII.

The residue of the filtration marked A weighed two drams, was of a blackish-grey colour, and had scarce any taste: it was divided into two equal parts, one of which boiled with 32 parts of distilled water, lost only 33 of its weight; the rest remained insoluble in water. The filtrated solution comported itself in the following manner. 1. It did not render the solution of glue turbid. 2. Mixed with an acid, and afterwards with the solution of glue, it gave the known insoluble precipitate. This solution was therefore the same as that of Experiment VII. excepting that it was astringent to the taste, and its colour not green, but greatly resembling that of a decoction of galls.

Experiment IX.

Upon the residue which, after the ebullition (Experiment VIII.) had remained upon the filtre, some strong nitric acid was poured, in which it was dissolved very rapidly with a gentle heat, and the solution assumed a superb red colour. On the application of a stronger heat, nitrous acid was formed, and the liquid assumed a pale yellow tinge, like white wine. This liquid did not render the solution of glue turbid, not even when the free acid had been saturated with ammoniac; nor did the slightest reaction manifest itself with regard to the red muriate of iron, either before or after the saturation of the free acid with ammoniac. But when oxalate of potash was added to it, a considerable white precipitate was formed, which, having been heated to whiteness, in a crucible of platina, comported itself like pure lime.

Experiment X.

The other half of the boiled residuum was heated to redness in a crucible of platina with admission of the air, and

and it was with difficulty incinerated; the greenish-white residuum was heated with nitric acid, and was perfectly dissolved, except a few particles of coal. The carbonate of potash precipitated from this liquor, when filtrated, a white powder, which had all the properties of carbonate of lime.

Experiment XI.

The dry residuum of Experiment VII. marked B, became somewhat humid in the air, and was easily redissolved in water, though there were some insoluble particles in it. The solution had an alkaline taste, and manifested no re-action upon that of glue till after it had been saturated by some acid. It was only by this means that the known precipitate was obtained. Heated with nitric acid, it was completely decomposed; at first red, afterwards of a pale yellow, it manifested no more action upon the solution of glue, nor upon the saline combination of iron; and even the oxalate and the carbonate of potash were unable to throw down any precipitate from it.

The inferences which I have drawn from these experiments are as follows. 1. The tannin prepared in the manner indicated is not yet pure, but contains lime, (probably in combination with some acid), as appears from Experiments IX and X. 2. The precipitate obtained by the addition of carbonate of potash is by no means pure tannin, but a mixt, composed of potash, lime, and tannin. 3. A considerable part of the tannin remains in solution in the liquid, and appears to be in a chemical combination with the potash. 4. On this account the new process of Proust, which consists in precipitating the tannin from an extract of galls by carbonate of potash, is by no means admissible, and that chemist is mistaken in considering this precipitate to be pure tannin; and

and even the explanation, which supposes the precipitation of tannin by potash to consist in the subtraction of the water effused by the latter, does not appear to me to be at all founded in truth; for, were this the case, the precipitation of the tannin would be effected equally by evaporation, or by the intermediation of alcohol, which by no means happens.

It seemed to me to be also necessary to examine the effect of potash in its caustic state, for which purpose I made the following experiments.

Experiment XII.

Upon one part of my tannin, dissolved in six parts of distilled water, I poured slowly, drop by drop, a pure and concentrated caustic ley. When the whole had acquired the consistence of a thick soup I put it upon the filtre. The small quantity of liquid that passed was of a pale brown colour; on the following day it had assumed a deep green tinge, and the whole filtre was of a black-green. Having collected what remained upon the filtre, I washed it six successive times in cold distilled water, and dried the residuum, which weighed only 0,20 of the tannin employed, and which, after having been triturated, presented a powder of a brown coffee-colour. Heated in a crucible of platina, this residuum did not fuse; but, on the contrary, began to be consumed with a light flame, and was incinerated not without great difficulty. The ash obtained was without taste, dissolved with effervescence in nitric acid, from which it was precipitated by the oxalate of potash, and the precipitate having been heated to whiteness, yielded pure lime.

Experiment XIII.

The liquor which had been poured upon the residuum (Experiment XII.) having been filtrated, was evaporated

to dryness; and during the evaporation there was still formed a small quantity of precipitate. Hence resulted a mass, which dissolved with great facility in water, giving it a dark-brown colour, and which did not alter animal jelly; but, united with an acid, it effected its decomposition, and, in a word, comported itself in every respect like the mass of Experiment VII.

Thus caustic potash produced the same effect as the carbonate of potash; that is to say, there resulted from its action a precipitate charged with lime, and the liquor contained tannin united with potash.

I now judged it a matter of the highest importance to separate the lime from the tannin, for it might easily happen that it, or the calcareous salt, had upon the different phenomena an influence which was formerly attributed to the pure tannin. I thought I could not better accomplish my purpose than by mixing the solution of tannin with water of barytes, expecting thus to decompose the calcareous salt, and afterwards to separate the barytes that might remain in the liquid by the sulphuric acid, &c. But I was disappointed in my expectation, as will appear from the following experiment, because the barytes precipitated the whole of the tannin.

Experiment XIV.

Having dissolved a part of my tannin in six parts of water, I poured into it water of barytes until no more precipitate was thrown down. The water above the precipitate was perfectly colourless, and scarcely contained the slightest trace of tannin. The dry precipitate was of a greenish-grey, and dissolved with ease in the nitric acid, from which some black flakes, of a resinous nature, were disengaged. There was also formed a residuum, which was insoluble in water. Having boiled it with carbonate

carbonate of potash, in a small glass, I poured it upon the filtre, upon which a white powder attached itself; which, after having been washed, effervesced strongly with muriatic acid, and dissolved. Sulphuric acid poured into this solution rendered it very turbid. These few experiments, which the small quantity of matter did not permit me to pursue farther, led me to conclude that this undissolved residuum was sulphate of barytes. The orange-coloured solution in the nitric acid yielded, by the addition of sulphuric acid, an abundant precipitate of sulphate of barytes which had been formed.

Experiment XV.

According to Proust, the salts which attract moisture separate pure tannin from its aqueous solution. I put into a concentrated solution of tannin, half its weight of dry and pure muriate of lime, and stirred the whole together. The mixture became very turbid, and at the end of two hours I put the whole upon a filtre. The liquor passed as highly coloured as before, and still reacted with the same energy upon the solution of animal jelly, as I convinced myself by a comparative experiment. I poured cold water upon what remained on the filtre till it passed colourless, which required only three waters. Desiccation left a light silky, shining, and insipid substance, which exhibited the following properties.

1. Cold water attacked it but feebly, and it required a very considerable quantity of hot water to effect its solution.

2. This solution was not rendered turbid by lime-water.

3. The muriate of barytes threw down a considerable precipitate insoluble in the nitric acid.

4. The oxalate of potash produced an abundant precipitate, which was speedily dissolved in the nitric acid.

5. Fire did not produce the slightest change in this white precipitate.

6. Mixed with charcoal powder, and heated to redness in a small crucible, it exhaled an odour of acid of hydrothion when, after it had cooled, some muriatic acid had been poured upon it.

These experiments seem sufficient to prove, that the substance collected was only calcareous sulphate; and thus we may now know in what state the lime exists in the tannin. The separation of the calcareous sulphate by the muriate of lime, consists certainly in the attraction of the solvent, though in fact it is very surprising that the calcareous sulphate does not separate when the astringent matter is dissolved in the smallest possible quantity of water, or that it does not form into crystals during slow evaporation.

Experiment XVI.

I dissolved a part of my tannin in eight parts of water, and then poured into it some drops of carbonate of potash. After having collected the precipitate, I poured into the filtrated liquor acetite of lead till no more precipitate was thrown down, and the water above was clear. The precipitate thus obtained was well washed with distilled water, and afterwards dried. The precipitate obtained by the carbonate of potash yielded, by dissolving in nitric acid, with a disengagement of nitrous gas, a liquor of a deep red colour, from which lime was precipitated by the well-known means. The lead charged with tannin, having been dried, was reduced to fine powder, and mixed with distilled water in a flask of Woulfe's apparatus, into which I conducted a large quantity of gas

of

of acid of hydrothion. The water gradually became coloured, and assumed the tinge of a concentrated solution of tannin. The liquid, after boiling some moments, was filtrated cold. The hydrothion of lead which remained upon the filtre was still treated twice in the same manner, after which nothing more was disengaged from it. All the lixivia were afterwards evaporated to dryness by a gentle fire, and yielded a mass, which I consider at present to be pure tannin; for, though its solution with the carbonate of soda yielded a precipitate, no more lime could however be found in it; else it dissolved perfectly in distilled water, and I observed also that the tannin obtained in this manner was more easy to dissolve in water than it was before. The sulphurated hydrothion of lead was of a black colour, and interspersed here and there with yellow spots of sulphur; heated in a close vessel, sulphurous acid was disengaged from it, and sulphur sublimed; after which an empyreumatic liquor rose, which proves that there was still tannin in it which could not be separated. This is then the manner in which we may at present obtain the astringent principle pure, though with loss. Nevertheless, I am still in some doubt whether the acid of hydrothion had not some pernicious influence upon the separation of the tannin.

Experiment XVII.

On this account I dissolved in distilled water a part of my tannin still impregnated with sulphate of lime; after which I conveyed into it, for a space of time, streams of acid of hydrothion. The liquid having been heated, filtrated, and evaporated, restored the tannin without alteration. Thus the acid of hydrothion manifested no effect upon the tannin.

I do not know whether all galls contain sulphate of lime; I rather believe the contrary to be the case: however, the experiments which I have just recorded prove that it was contained in those which I used. Having employed no water but what was distilled, it is evident that it cannot be attributed to that source.

Some may perhaps be inclined to ask, why I took so circuitous a method in order to obtain tannin in a pure state, when Proust has indicated more expeditious processes for this purpose? What determined me to do so was, that I was, in the first place, convinced, from my own experiments, that several of the methods indicated did not afford tannin in a state of purity; and, in the second place, I also mistrusted the other processes. The first method published by Proust consists in precipitating the tannin from a decoction of galls by the muriate of tin, and decomposing the tannate of tin by the acid of hydrothion, &c. In the sequel Proust found himself that the precipitate produced by a solution of tin in the extract of galls is a combination of gallic acid and tannin with oxyd of tin. The second method of obtaining the tannin was to consist in decomposing a concentrated infusion of galls by carbonate of potash. But, it has been proved, (Experiment XI.) that such a precipitate is nothing less than pure tannin; and I shall have occasion to speak again on this subject. According to Proust, the tannin may also be separated from its aqueous solution by the salts which have a great affinity for water, which, however, has never succeeded with me. Finally, the sulphuric acid is also said to be a means of effecting the separation. In that case, sulphuric acid is to be poured into a good extract of galls till no more precipitate is thrown down, the liquid then clarified, the precipitate washed with cold water, afterwards dissolved in boiling

boiling water, and the sulphuric acid saturated by carbonate of potash; in this manner the tannin is precipitated in proportion as the sulphate of potash formed dissolves in the water, &c. This method of proceeding would in fact be very convenient, were there not reason to fear that the sulphuric acid changes the nature of the tannin; and this is what I determined to ascertain by experiment.

Experiment XVIII.

Eight ounces of galls, carefully selected, were reduced to a very fine powder, and infused cold in between eight and ten pounds of distilled water, which were afterwards reduced by evaporation to eighteen ounces. Into this was poured, drop by drop, concentrated sulphuric acid, diluted with one half of distilled water, which produced an abundant precipitate, of a milky white, much resembling curdled milk, which soon became brown, depositing a mass of a resinous appearance. I added acid till nothing more was precipitated, so that the acid was in excess in the liquid, which was of a pale yellow, and retained hardly any astringency. At the end of twenty-four hours the precipitate had strongly attached itself to the bottom of the vessel, and presented entirely the appearance and consistence of the resin of jalap, such as it is found at the bottom of the vessel when the alcohol has been drawn off from it. A part was dissolved very easily in alcohol, as also in boiling distilled water. Cold water seemed to attack it but feebly; on which account I lixiviated it twice with cold distilled water, which I decanted, and put by. After this I poured upon the residuum about eight ounces of boiling distilled water, which immediately gave a fine transparent solution, resembling a highly-concentrated infusion of galls. Its taste was scarcely acidulous, so that the sulphuric acid seemed

seemed to have entered into an intimate combination with the tannin. Into this solution, whilst still hot, carbonate of potash dissolved in water was cautiously poured till no more effervescence was observed. Considering the quantity of sulphuric acid employed for the precipitation, that of the potash appeared to me too insignificant; hardly had two drachms of the alkaline solution been poured into it, when the effervescence already ceased, and yet I did not venture to add more, for a sample into which I poured some more drops of potash, yielded a liquor which was soon turned green by the contact of the air; a property which is found in tannin united with the alkali, and of which I shall say something hereafter. When the solution united with the alkali was cooled, there was found at the bottom of the vessel a considerable quantity of friable matter, of a blackish brown colour, swimming in a turbid liquor, of a brownish yellow colour. I put the whole upon a filtre; and, after having let the liquid-part run off, I marked it with the letter A, and lixiviated the rest with boiling distilled water to perfect solution. This lixivium ought then to have contained the pure tannin; but, as it was to be presumed that there was still sulphate of potash in it, I endeavoured to separate it by the following experiment.

Experiment XIX.

This lixivium having been evaporated to dryness, the residuum was triturated in the same vessel, and mixed with about six times its weight of alcohol, at 0,85 specific gravity. Though pure tannin is insoluble in alcohol, rectified to the highest degree, yet it dissolves in it very easily when the latter contains some hundredth parts of water. As alcohol of the above-mentioned specific gravity still contains, according to Lowitz's table, twenty-three

three hundredths of water, a slight solution might have been expected; and as the sulphate of potash is not soluble in this spirituous vehicle, it ought to be separated. But the result was different from what I had expected; that is to say, the alcohol did not appear to act cold upon the dry extract; by the aid of ebullition it could dissolve a part of it, but there remained a considerable residue, which was no longer attacked by an addition of alcohol, and which, to judge by appearance, was nothing less than a simple sulphate of potash. It was a brown, spongy, friable mass, without any saline taste, though possessed of a slight bitterness and astringency. I collected it upon the filtre, after having suffered the alcoholic solution to run off, which I put by, reserving it for farther researches.

Experiment XX.

The mass which the alcohol had not dissolved was put into a porcellain vessel, and bathed with distilled water, which very quickly effected its solution. As this solution appeared still to redden the tincture of turnsole a little, I imagined that this mass might be a combination of a small quantity of acid and of tannin, although I entertained the opinion that I had sufficiently decomposed it. (Experiment XVIII.) I therefore still added to it some drops of a solution of carbonate of potash, and after the evaporation and desiccation I treated it anew with alcohol, combined with about thirty hundredths of water; but the result was only a slight tinge, and the matter remained unaltered upon the filtre. Before I subjected this residue to new experiments, I thought it necessary to examine whether the alcoholic solutions really contained tannin.

Experiment

Experiment XXI.

I therefore took all these spirituous solutions, (Experiments XIX and XX.) sufficiently saturated in colour; I poured upon them a fourth of distilled water, I distilled the alcohol from them by a retort, and then evaporated the rest of the liquor to dryness. I obtained a mass entirely resembling pure tannin, having a very astringent taste, but which dissolved with a little more difficulty than the tannin prepared before, (Experiment XVI.) I could not exactly ascertain its weight, because during the operation some of it had been employed for various little experiments.

Experiment XXII.

The mass not dissolved by the alcohol, (Experiment XX.) which might weigh about the half of the tannin obtained pure, was boiled in eight times its weight of distilled water. It was perfectly dissolved, with the exception of a small residuum, which remained upon the filtre. The filtrated liquor was of a dark brown colour, of little astringency, and did not act upon the paper of turnsole, whether blue, or reddened by the acids, nor upon paper tinged yellow by saffron. I poured a small quantity into a solution of fish glue, but no precipitate was formed; the addition of an acid produced in it the well-known coriaceous combination. The solution of carbonate of potash and of caustic potash, mixed with the liquor, produced no change in it; but as soon as I had added some drops of sulphuric, nitric, or concentrated acetic acid, the mixture always became turbid, and a flaky precipitate was produced, which was not dissolved again by the addition of a large quantity of acid. When I collected the precipitate upon a filtre, the liquor passed through of a very deep colour, like an infusion of galls, and

and strongly precipitated the solution of animal glue. The muriate of barytes produced in this liquor a precipitate, which the nitric acid could not re-dissolve, and which I discovered to be sulphate of barytes. This composition therefore actually contained sulphate of potash, besides tannin, which probably was combined with the potash, since it did not act upon the solution of glue till after the addition of an acid. But what was the third substance which was thrown down to the bottom of the vessel when acid was added to the liquor? In order to ascertain this, the following experiment was performed.

Experiment XXIII.

All the liquor that remained from these experiments (XX.) was mixed with muriatic acid, and filtrated, as soon as it appeared no longer to deposit any thing. What remained upon the filtre was washed several times with cold distilled water. The filtrated liquor yielded by evaporation a mass, which gave at the bottom of the vessel some crystals of the same nature as the muriate of potash; moreover, the brown mass had all the characters of tannin. What had remained upon the filtre diminished much in drying, and gave a black, brilliant, and very friable powder, which exhibited the following properties.

A. It had scarce any thing of a bitter taste.

B. Highly-rectified alcohol heated with it turned yellow without dissolving much of it.

C. A mixture of two parts of alcohol and five of water produced, with the aid of ebullition, a perfect solution.

D. One part was dissolved very rapidly in twenty parts of boiling water.

E. This solution did not redden the tincture of turnsole.

F. It did not precipitate the solution of glue, not even with the addition of an acid.

G. The muriate of iron was thrown down by it in a black precipitate, with large flakes, which soon fell down to the bottom, and the liquor assumed the transparency of pure water.

H. The nitrate of potash was thrown down by it in a brown precipitate.

I. The muriate of tin was scarcely rendered turbid by the solution.

K. Caustic potash in a liquid state produced speedily a concentrated solution of a brownish red colour, which did not turn green in the air, and which, by means of saturation with the acid, restored, after cooling, the dissolved matter.

These experiments led me to to consider this mass as tannin a little altered; for it was not gallic acid, not acting upon turnsole paper, and not dissolving in pure alcohol: it differed from ordinary tannin in its producing no effect upon a solution of glue. I am inclined to believe that it is a substance intermediate between tannin and gallic acid.

Experiment XXIV.

I then examined the liquor marked A, which I had put by from Experiment XVIII. It was of a grass-green colour, and there was seen floating in it a substance of a fatty appearance, but in too small quantity to be more particularly examined. This liquor did not precipitate the solution of glue; but when an acid was added to it (I generally employed the muriatic acid for this purpose) there was formed a very abundant coriaceous precipitate, but of a very dark colour. The green colour led me to believe that the tannin, which was here in combination with a small excess of potash, was a little oxydated; I therefore

therefore caused acid gas of hydrothion to pass into the liquor till the colour again appeared such as is presented by an ordinary decoction of galls, which required only a few seconds; I then added some drops of pure acetic acid, and afterwards poured nitrate of tin into it till a precipitate was formed, which was well washed, collected upon a filtre, and dried. I must here correct a mistake of Proust. That chemist considers the green liquor to be gallic acid combined with potash and oxydated; but the pure gallic acid gives with a pure alkali a liquor which indeed becomes somewhat brown in the air, but never assumes the green tinge. Pure tannin, on the contrary, which is not yet altered, gives with potash a composition which acquires a red tinge by oxydation.

TO BE CONTINUED IN OUR NEXT.

Examination of the different Processes for parting Nickel and Cobalt. By C. F. Buchholz.

From the Annales de Chimie.

BEING in want of nickel and of the oxyd of cobalt in a state of purity, I was induced, in order to obtain them, not only to make some experiments of my own invention, but also to repeat different processes that have been proposed for the same purpose. As it happens but too frequently that accidental circumstances are omitted by the authors of these processes, or that they do not employ, and often indeed cannot, on account of their ignorance of the indicative characteristics, the most scrupulous accuracy in the examination of the products obtained, (on which account the method followed furnishes itself a testimony of its insufficiency,) it will undoubtedly be acceptable to che-

mists to find here a succinct account of my experiments upon this subject. The result which they have afforded me, placed in comparison with those obtained from the labours of other chemists, will at length point out the most efficacious means of attaining the end proposed, and at the same time enable others to avoid fruitless labour and expense.

A. That ingenious chemist M. Hermbstadt had proposed, for separating the oxyd of cobalt from the oxyd of nickel, a method which consisted in dissolving in ammoniac the nitrate or the sulphate of cobalt, impregnated with nickel, and exposing the solution to a single evaporation. I resolved to try this process for the contrary purpose.

1. I therefore dissolved an ounce of earthy cobalt ore in four ounces of hot nitric acid, of the specific weight of 1,220, to which an equal quantity of water had been added; which gave a residuum of three drachms of oxyd of arsenic, in the form of small crystals. When the solution, mixed with one half of water, and of a dirty green colour, had been filtrated and diluted with a larger quantity of water, a small quantity of oxyd of bismuth was separated. Caustic ammoniac was mixed with it in excess, till no more sensible solution took place in the precipitate obtained. What was not dissolved was of a dirty reddish white, and was a compound of arseniate of cobalt with a small quantity of oxyd of bismuth and of oxyd of iron.

The filtrated solution, which was of a fine blue colour, was evaporated by a gentle fire, by means of which there was obtained about two drachms of a precipitate, of a fine light green colour, which, on examination, was found to be an oxyd of nickel combined with oxyd of cobalt. The filtrated liquor having then been evaporated by

by the heat of a stove, still deposited oxyd of the same quality. The saline mass of ammoniated nitrate of nickel, of a pretty dark green colour, which had been obtained by the evaporation, was dissolved again, filtrated, and kept boiling with an excess of caustic potash till the complete evaporation of the ammoniac; by means of which there was separated still a drachm and a half of oxyd of nickel, which appeared now to contain but very little oxyd of cobalt.

2. As the parting was not effectuated very well nor easily by the process just mentioned, I made a trial of the sulphuric acid. For this purpose I poured an equal part of water upon the oxyd obtained before, and added sulphuric acid till, with the aid of heat, the whole was dissolved. There was then disengaged, in a very evident manner, an odour similar to that of oxygenated muriatic acid, though not a single particle of muriatic acid was employed. I have already observed the same phenomenon on a former occasion, and without having been able hitherto to pay more particular attention to it, I communicated it to the public. (Beytraege zur Erweiterung, &c. No. I, 1799, p. 18.) The solution was then treated with ammoniac as before, till almost the whole was redissolved; the residue, which was an oxyd of cobalt with a small quantity of oxyd of nickel, had the colour of verdet. When the solution had been evaporated by a graduated heat, and separated by filtration from the powder deposited, of which the greatest part was still oxyd of cobalt, it was left to spontaneous evaporation. It crystallized, without any separation, in prismatic crystals accumulated together; part of which were of a pale green, and the rest in crusts, adhering to each other, and of a blue colour at the margin. On examining the oxyds obtained by potash from the solution of these crystals, as well

well as the mother waters, they were found to contain, the one and the other, cobalt nearly in the same proportion.

3. Though in this case the result was not yet what might be wished, I nevertheless resolved to recommence the same experiment upon a larger quantity, hoping that the parting might be better effected by crystallization. In consequence, the oxyd separated by the carbonate of potash from a solution of eight ounces of earthy cobalt ore in common nitric acid, (which had previously been evaporated, repeated, and filtrated); this oxyd, treated with a sufficient excess of potash to separate from it, as much as possible, the arseniac acid that might be combined with it, was dissolved in sulphuric acid, diluted with eight parts of water; and pure ammoniac was added to the liquid till the whole of what was soluble in the precipitate was dissolved. The filtrated solution was evaporated by gentle boiling, and then left to spontaneous evaporation, after having been freed from a small quantity of cobalt mixed with nickel of a pale green colour. The saline part collected gradually in crystals of a blueish green, which were perceived here and there to be prisms, which at different points were charged with slight crystals of calcareous sulphate. In order to separate these crystals as much as possible from the mother water, they were washed with distilled water, and dried between leaves of blotting-paper. Though the rest of the lixivium did not show any inclination to deposit any more regular crystals, yet I could not discover any difference between the metal which it contained and that of the crystals; they were both an oxyd of nickel mixed with cobalt. The crystals, which amounted in weight to two ounces and a half, were dissolved again in thirty-two ounces of boiling water; the solution was evaporated

rated till a slight pellicle formed, and, after having been filtrated, it was placed near a stove, to cool gradually and crystallize. At the end of forty-eight hours the greater part of the salt was crystallized in fine short pyramids, of a tetrahedral rhomboidal form, and of a yellow-green colour; the lateral faces of which formed an angle of 115 and 65 degrees, frequently with a truncated extremity, and always with an angle of 132 degrees inclination towards the terminating face. This result proves, that this salt can form more easily into regular crystals by cooling than by slow evaporation. All the crystals collected, having been washed with water, were dissolved again; and the separation of the oxyd of nickel was affected by the carbonate of potash, which was boiled, in order to disengage the ammoniac.

Both in order to obtain this oxyd without carbonic acid, and to judge whether it was freed from cobalt, I dissolved it in nitric acid, and afterwards heated it with ammoniac, in the manner that has already been several times mentioned. I evaporated to dryness the fine blue liquor that had been separated by filtration from a residuum of five grains, which comported itself like an impure oxyd of cobalt. After another solution, there was deposited an oxyd of a beautiful light green colour, which, having been washed and dried, weighed half an ounce. The liquor which passed through the filtre was analyzed by pure carbonate of potash at the heat of boiling water, which gave still 170 grains of oxyd of nickel, containing carbonic acid, and of a pale green colour. I dissolved a small quantity of it in muriatic acid, and poured some of the solution upon paper. On heating it afterwards, the spot became yellow, inclining but very little to green. But the oxyd of nickel, which had separated spontaneously during the evaporation, was dissolved

dissolved with a disengagement of much oxygenated muriatic acid, and, poured upon paper, it exhibited, after having been heated, the colour of a highly-saturated sympathetic ink of cobalt; whence it follows, that it was richer in cobalt than that collected from the precipitation.

The oxyds collected in both ways dissolved in the nitric and sulphuric acids, after having become grey, a phenomenon which I have mentioned elsewhere. I imagined that this proceeded from the circumstance, that the oxyd of nickel had perhaps been dissolved the first, and that the oxyd of cobalt had, at least for the greater part, remained the last; which however was not confirmed by the experiments made on this subject. Heated to a slight redness, these oxyds changed their colour to a blackish grey, and then, as also by the addition of sulphuric acid, there was disengaged from the residuum obtained by evaporation, nitrous acid, which was also separated from it by the addition of an alkaline ley; for the rest, it comported itself with ammoniac, &c. in the manner which I have already indicated.

From what has hitherto been said it results:

a. That the sulphates and nitrates of ammoniacal nickel, extracted from the cobalt ore, always retain still cobalt in their composition, and it is impossible, according to the process of Hermbstaedt, modified in the manner above mentioned, to obtain the oxyd of nickel without admixture of cobalt.

b. On partially decomposing the nitrate of ammoniacal cobalt by evaporation, we obtain an oxyd of nickel very rich in cobalt, which contains nitric acid; and the oxyd of nickel which is found in this salt, not yet decomposed, contains a much smaller quantity of cobalt.

B. Doctor

B. Doctor Schnaubert has indicated (Tromsdorf's Journal of Pharmacy, vol. II. No. 2, p. 66.) a process for obtaining an oxyd of pure nickel; which consists in dissolving the metal of nickel mixed with cobalt, or its oxyd already disengaged from the other substances in nitric acid, precipitating it by carbonate of potash, and heating it to whiteness after the washing and desiccation. In this manner he always obtained a yellow oxyd upon which he afterwards boiled pretty strong sulphuric acid, which gave him a solution of nickel of a grass-green colour, whilst the oxyd of cobalt presented itself as a residuum of a yellow colour. He proved the purity of the sulphate of nickel prepared in this manner by the property which ammoniac possessed of precipitating it of a light green, and, added in excess, of redissolving it with a fine dark blue colour. This argument will appear insufficient to whoever knows that the oxyd of nickel, mixed even with several hundredth parts of cobalt, nevertheless experiences no sensible change of colour in its precipitates, nor in its solution with ammoniac. Besides, the want of an indication of the means whereby he convinced himself that the oxyd obtained as residuum from the solution in sulphuric acid was really an oxyd of cobalt; the vague direction to heat the oxyd composed, without the least instruction respecting the degree of heat to be applied, the uncertainty which he leaves with regard to the strength of the sulphuric acid that is to be used; all these circumstances create a doubt respecting the accuracy of the process indicated, which the following experiments may be able to clear up.

1. A portion of the carbonic oxyd of nickel, A 4, was exposed for the space of an hour to an intense red heat, almost raised to the white. The oxyd, whilst still hot, was of a brownish yellow; after cooling it assumed a grey colour, inclining to the yellow, but not yellow. The

oxyd of nickel obtained by evaporation A 4, having been treated in the same manner, was still a little more grey than the preceding. The carbonic oxyd of nickel was again exposed for half an hour longer to a degree of heat raised to whiteness: whilst still hot, it was yellow, inclining to brown; but when cold it was grey, inclining to a brownish yellow.

2. Thirty grains of this oxyd, heated to redness, were put to digest for some hours with ninety grains of pure sulphuric acid, of 1,860 specific weight. Having afterwards been heated, the mass immediately puffed up with a sound of explosion, and presented a yellow substance, inclining to the green. By means of ebullition with half an ounce of water it was dissolved, excepting a grain of powder, of a yellow-grey colour, which appeared to be an oxyd of nickel, mixed with cobalt, together with a small quantity of dross. I obtained precisely the same result, wth the same phenomena, on heating a second time in the same manner, and with ninety grains of concentrated sulphuric acid, thirty-five grains of oxyd of nickel, which I had obtained by heating briskly to redness sixty grains of ammoniacal nitrate of nickel, prepared by evaporation. By heating the same oxyd even to whiteness, under the bellows, for the space of half an hour, I did not obtain a yellow mass, but one of a yellowish grey, inclining a little to the green, which comported itself with the sulphuric acid in the manner I have already specified.

3. I then repeated the experiment with dilute sulphuric acid: 160 grains of carbonic oxyd of nickel, which had been precipitated from several solutions, were exposed, for the space of half an hour under the bellows, to the most intense white heat, after which they still weighed seventy-five grains. This substance was here and there of a greenish yellow, and of a blueish grey

at

at the points of contact with the crucible; being pulverized, it yielded a powder of a blackish grey. It was mixed with a drachm of sulphuric acid, diluted with five drachms of water: immediately a brisk disengagement of gas took place, and, on afterwards heating this mixture, a smell of hydrogene gas was evidently perceptible. After sufficient ebullition, some more water was added, and the solution was poured off clear. The residuum was heated again with dilute sulphuric acid; this yielded them ten grains of a residuum which was nothing less than oxyd of cobalt, but an oxyd of nickel mixed with cobalt, as was proved by its solution in the acids and ammoniac. The two preceding solutions were analyzed, each separately, by pure potash, and the precipitate was besides heated with an excess of potash, and afterwards washed and dried. On being examined, each of these precipitates presented cobalt, which was always found more pure in that of the first solution; for the solution in muriatic acid, being poured upon paper and heated, inclined sensibly to the yellow, whilst the precipitate of the second solution gave a pure light green tinge. It is astonishing that the precipitate of the first solution yielded more oxygenated muriatic acid than that of the second.

The experiments which I have recorded, and other similar ones, of which I have made no mention, prove:

A. That the oxyd of nickel, heated gently or violently, does not assume the yellow colour; and that, if this colour has been observed by M. Schnaubert, it must have depended either upon a certain proportion of the elements which entered into the composition of the oxyd, or perhaps upon the admixture of a small quantity of arsenic.

B. That we cannot, by means of M. Schnaubert's process, obtain an oxyd of nickel exempt from cobalt, as it

does not even effect a parting of the two oxyds sufficiently complete to be sensible to the eye.

C. I pass over in silence several experiments which I have made, in order to discover a certain and accurate method of effecting this separation, because they have not led me to the desired end, nor presented any other interesting phenomenon. They principally tended to discover an acid which, with one of the oxyds, should form an insoluble salt, and with the other a salt of easy solution. I was therefore obliged to return to the process indicated A 4, which consists in a partial decomposition of the ammoniacal nitrate of nickel; for that which has been proposed by Lehmann, (Cadmiologia, Part II. page 110,) of fusing from fifteen to twenty times, to incipient vitrification, the nickel mixed with cobalt, in order to scorify the whole of the cobalt; as also that indicated by Bergman, (Opuscul. Physic. & Chem. vol. II. p. 246—249,) of repeating the fusion three or four times with from eight to twelve times as much pure nitre, were too laborious and expensive. I therefore repeatedly heated the oxyd of nickel, which had been separated by means of the carbonate of potash from the triple salt not dissolved by the first evaporation, in such a manner, that after having dissolved it in nitric acid, I had recourse to ammoniac and to evaporation, as I have indicated above. In this manner I finally obtained, completely free from cobalt, the oxyd separated by the potash from the triple salt, which had been dissolved again after the evaporation: this oxyd had the properties indicated in a memoir printed in the second volume of the Chemical Journal, published at Berlin by Buchholz, Crell, Hermstadt, &c.

The oxyd which had been separated by evaporation from the nitrate of ammoniacal nickel, was, in the last operation, already quite freed from cobalt; it only contained

tained still, as has been observed, a small quantity of nitric acid. The oxyd of nickel which, after having been left naked by evaporation, still contains cobalt, is naturally susceptible of undergoing the same operation anew.

This method may be employed till farther researches have made us acquainted with one more expeditious; for it is attended with no considerable expense, since, by means of potash, we may perform in a retort the evaporation of the ammoniacal nickel, as also the subsequent decomposition of the triple salt, and thus obtain the ammoniac for other uses; as we may also, in operations on a large scale, obtain a part of the nitre from the last operation, by the evaporation of the water of lixiviation.

Extract of a Memoir on Milk. By M. THENARD.

From the BULLETIN DES SCIENCES.

IN a memoir, which I read to the Philomathic Society in Priarial, year 12, I have shewn that milk always contains free acetous acid in greater or less quantity. At the same period Messrs. Fourcroy and Vauquelin found that it contained also phosphate of magnesia, and besides, that the lactic acid of Scheele, or that which is extracted from the serum of milk spontaneously coagulated, was itself nothing else than the acid of vinegar, combined with an animal matter. In the present state of the science, therefore, milk is to be considered as a compound, 1, of water; 2, of acetous acid; 3, of cheesy matter; 4, of butyraceous matter; 5, of sugar of milk; 6, of extractive matter; 7, of muriate of soda and of potash; 8, of sulphate of potash; 9, of phosphate of lime; 10, of phosphate of magnesia.

Of these eleven matters there is one which I particularly examined some months ago, namely, the cream. It

was

was my wish to determine what the circumstances were which operated in its separation, and especially in its transformation into butter.

I had already observed that milk coagulated equally well in close vessels as in open ones; I knew that in this decomposition no gas was disengaged, and that, in order to produce it rapidly, it was sufficient to raise the temperature to between 20° to 40°. I therefore considered it as demonstrated, that the air contributed neither to the formation nor to the separation of the cream, and that it existed ready formed in the milk; but it still remained for me to ascertain the principles which entered into its composition. Persuaded, from different observations of my own, that it was nothing else than an intimate mixture of butter, cheese, and serum; in order completely to ascertain this point, I filled a quart bottle, almost up to the neck, with fresh cream, and displaced the air which remained in it with carbonic acid; having then corked it well, I shook it strongly in every direction for the space of half an hour; at the end of this time the matter, having become very thick, and adhering strongly to the sides of the bottle, detached itself gradually from them, and was soon after converted into a white liquid, in the middle of which floated a yellow lump of excellent butter. The butter consequently exists in the milk; it is separated from it when the milk, deprived of the vital action, is left to itself; then, either by the formation of an acid, which would undoubtedly be owing to the decomposition of the extractive matter, or perhaps by the specific weight of the butyraceous matter, being inferior to that of the cheesy matter, (for almost the very moment the milk is received into a vessel the butyraceous matter begins to separate); the milk is decomposed, the cream floats at top, and from the latter, by friction, and especially by the aid of a temperature between fifteen

to

to twenty degrees, we obtain butter and butter-milk; that is to say, a very mild white liquor, which is nothing else than serum holding in suspension butter and cheese in a very highly divided state; but the butter thus obtained is not pure. It still contains cheesy matter, sometimes even to the amount of a sixth of its weight. This is the reason why it so soon turns rancid, especially in summer; when, therefore, this matter is separated from it by melting, it acquires the property for keeping for a long time. In undergoing this fusion, indeed, it acquires an acrimony which greatly limits its utility, so as to admit only of its employment in frying; but this disadvantage would be remedied if the temperature were much less elevated. This was first observed by Clouet; and accordingly, in order to purify the butter, or to separate the cheesy matter from it, without giving it a bad taste, the following process must be followed.

1. Fuse it on the water-bath, or in a heat of 66° Reaum. at most; 2, keep it melted till the cheesy matter is collected in white flakes at the bottom of the vessel, and the supernatant liquor is clear; 3, decant it, or pass it through a linen cloth; 4, cool it in a mixture of equal parts of pounded ice and sea-salt, or, where ice cannot be procured, in spring water, using vessels of great width and little depth. Without this precaution the butter would become clotted and crystallize, and then it would no longer be fit for the use of the table. Moreover, the parts being thus brought into closer contact, resist the action of the air much better; for the same reason also the pot which contains it ought to be accurately closed, and placed in a cool cellar; by these means the butter may be kept for six months and more, and at the end of this time may be used, especially the second layer, almost like fresh butter. It is even possible to give this melted

melted butter almost all the appearance of fresh butter by beating it up with a sixth of its weight of cheesy matter; and rancid butter may likewise be rendered much more palatable by melting and heating it according to the process which I have just described.

List of Patents for Inventions, &c.

(Continued from Vol. VII. Page 480.)

JOHN SYEDS, of Fountain Stairs, Rotherhithe Wall, in the county of Surrey, Mathematical Instrument-maker; for a steering amplitude, or azimuth-compass and scale, for finding and working courses of ships.
Dated October 7, 1805.

DANIEL DESORMEAUX, of Barking, in the county of Essex, Surgeon and Apothecary, and SAMUEL HUTCHINGS, of Ilford, in the said parish of Barking, Weaver; for certain improvements in the making and manufacturing of wax, spermaceti, and tallow candles.
Dated October 22, 1805.

RICHARD KENTISH, late Captain in the Cambridgeshire Militia, but now of Birmingham, in the county of Warwick, Esq.; for an armour-waistcoat, which is a sure defence against the bayonet, sword, pike, or any pointed instrument, and in many instances may prevent the wound from a musket-ball. Dated October 30, 1805.

JOSEPH HUDDART, of Highbury-Terrace, in the parish of Islington, in the county of Middlesex; for sundry new improvements in the manufacture of large cables, and cordage in general. Dated October 30, 1805.

SAMUEL MILLER, of Gresse-street, in the Parish of Saint Pancras, in the county of Middlesex, Engineer; for certain improvements on steam-engines.
Dated October 30, 1805.

THE

REPERTORY

OF

ARTS, MANUFACTURES,

AND

AGRICULTURE.

No. XLIV. SECOND SERIES. Jan. 1806.

Specification of the Patent granted to WILLIAM DEVERELL, *of Blackwall, in the County of Middlesex, Engineer; for certain Improvements on the Steam-Engine.*

Dated August 2, 1805.

With a Plate.

TO all to whom these presents shall come, &c. Now KNOW YE, that in compliance with the said proviso, I the said William Deverell do hereby declare, that my said invention is described in the drawings hereunto annexed, and the following description thereof; that is to say: First, the manner and form of constructing the fire-place, or, in other words, using iron for the boiler to lay on instead of brick-work or other composition. Secondly, the manner and way of using the steam. Thirdly, my improvement on the cold liquor pump. These are my improvements, which I proceed to describe as follows.

First, Fig. 1, (Plate IV.) the steam-boiler. A A A, three iron cylinders, used instead of brick-work: all these cylinders are to be full of water, and may be connected to the steam-boiler as shewn at B B B, or in any other way

most convenient. These cylinders may be made to answer any shaped boiler, and they can be cleaned easily out, by taking off the flanches at C C C. Instead of being in three detached cylinders, they may be cast whole, or made of wrought-iron, to form the fire-place, but which will be attended with more trouble, and such cylinders or cylinder may be made of any convenient shape or form: these cylinders may also be used to a very great advantage where large boilers are already set; and being applied to engines of various powers, will with the same fuel be capable of doing considerable more work with the steam arising therefrom. In the present mode of setting boilers, the brick-work underneath them is attended with frequent repairs, owing to the action of the fire upon them; nor is this expense the only inconvenience attendant upon them, the whole concern is for the time they are repairing wholly stopped: in short, these cylinders may be set to advantage under boilers or coppers of any description.

Fig. 2 is a section of Fig. 1.

Fig. 3 the end-view of Fig. 6. When the steam comes from the steam-boiler it will enter the cylinder A, Fig. 3; by means of a valye or cock opening it will press the piston down from A to B, in Fig. 6; it will then enter Fig. 4, which is an end-view of the steam vessel at the end of the stroke, which will be full of steam, of about eighteen pounds density, filled from the steam-boiler Fig. 1; but Fig. 4 will be alternately hereafter supplied from Fig. 6, and Fig. 7 will be supplied from Fig. 4. At the same time as the piston in Fig. 6 begins to descend, so will the piston in Fig. 7; for Fig. 4 will supply Fig. 5, which is an end-view of Fig. 7, with steam, at the same time as Fig. 6 receives the steam from the boiler. By so doing the two pistons will work alternately to, and again, by

means

means of valves or cocks opening and shutting as in the common engines. Fig. 4 may be twenty-six times the contents of the cylinder Fig. 6; the larger it is the more regular the pressure will be on the piston. If the steam be of fifty-four pounds density in the boiler, the difference of the two working cylinders may be as one to three; for, as three times eighteen are fifty-four, the smaller one will supply the larger one with steam of eighteen pounds density, or thereabouts; but the density may be varied, if thought proper, but I conceive the above proportions to answer the purpose best.

My improvement here is, that of the steam going from Fig. 6 to Fig. 4, and then from Fig. 4 to Fig. 7, from which it is condensed. By this means the engine will be so regular in its operations, that it will work pumps or any other machinery. The steam in the boiler, Fig. 1, being fifty-four pounds density, the cylinder, Fig. 6, at the end of the stroke, will be full of steam of the same, or nearly the same, density, and Fig. 4, being full of steam, of eighteen pounds density, or thereabouts, the communication being open between Fig. 6 and Fig. 4, the steam in each of these will be twenty pounds density, little more or less; but at the end of each stroke of the pistons, before the opening is made between Fig. 6 and Fig. 4, the steam will be in Fig. 6 fifty-four pounds density, or thereabouts; in Fig. 4 eighteen pounds density, more or less; and also in Fig. 7 eighteen pounds density, little more or less; therefore the medium pressure on the piston Fig. 6 will be thirty-five pounds, little more or less, on the inch; for the re-action will be at the beginning of the stroke about twenty pounds on the inch, and at the end about eight pounds density. The medium pressure of the steam on the piston, Fig. 7, will be about nineteen pounds on the inch, for at the beginning of the

stroke the pressure will be about twenty pounds on the inch, at the end of the stroke it will be about eighteen pounds pressure; if the steam-vessel, Fig. 4, be made larger, the difference at each end of the stroke will not be so great: were the steam let out at fifty-four pounds density to the open air from the cylinder Fig. 6, there would be but thirty-nine pounds upon the inch, for the atmosphere re-acting as a spring of fifteen pounds, or nearly so, upon the inch, leaves thirty-nine pounds, or thereabouts. By letting the steam pass from Fig. 6 to the steam-vessel Fig. 4, instead of letting it out to the open air, it loses four pounds, or nearly so, on the inch, but it gains on a piston, three times as large, about twelve pounds on the inch, and there being but half the steam to condense, as in the common way, there will be a gain of considerable value. If the friction of the piston be nine pounds on the inch in the small cylinder Fig. 6, there will be but thirty pounds on the inch in neat power, or nearly so, when the larger one will work about twelve pounds upon the inch neat power. The steam-vessel will be best made of wood, that the steam may not condense; or it may be made of metal, but wood is preferable; and it may be made of what figure or shape that may be found most convenient. The steam-vessel has a valve placed in some convenient part for regulating the steam. The two piston-rods may both be connected to the one end of the beam; the large cylinder should be shorter than the small one, or agreeable to that which is fixed nearest the end of the beam. The cylinders may also be placed in the steam-vessel Fig. 4, or in the steam-boiler Fig. 1, or in any other place that may be most convenient. If the large cylinder or piston, or air-pump, condenser or cold liquor pump, should be out of order, the small piston may still be worked by disengaging the large

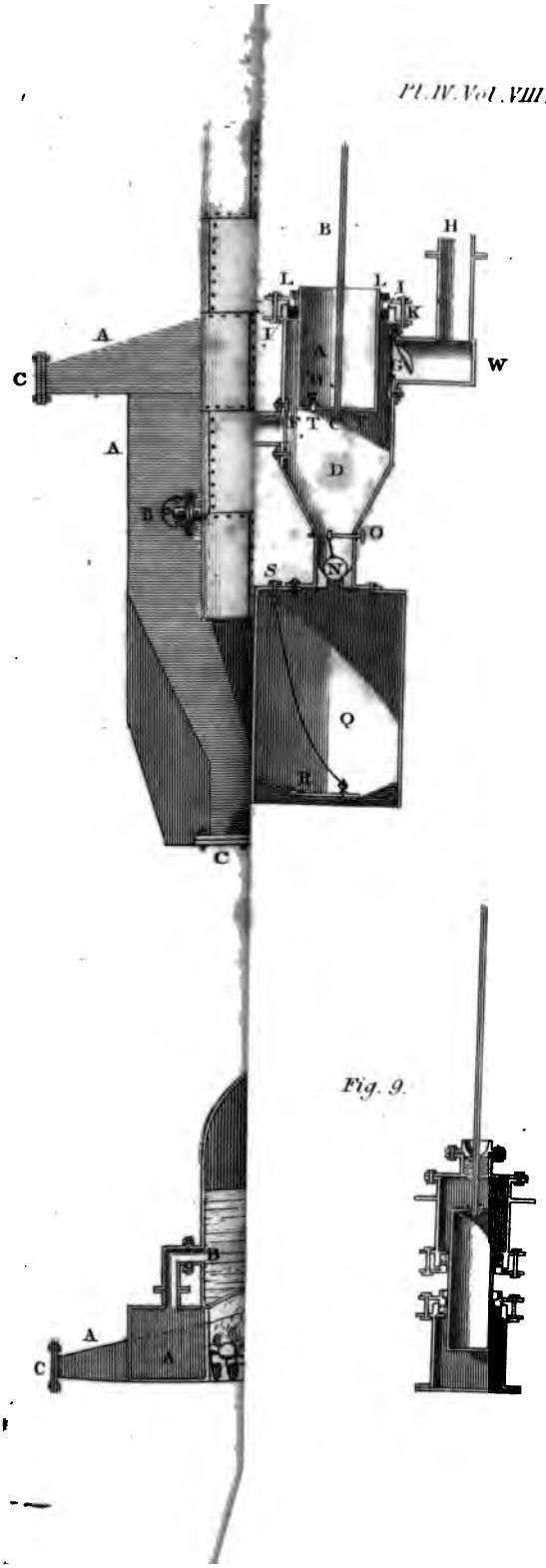


Fig. 9.

large piston from the beam; on the other hand, if the smaller piston be out of order, the large one may still be worked by disengaging the smaller.

Fig. 8, a section of the cold liquor pump. A, the piston. B, the rod. C, where the rod is made fast to the piston A. D, the pump, barrel, or cylinder. E, the feed-pipe. F, the under-valve. G, the delivering-valve. H, the delivering-pipe. I I, the packing round the piston. K K, two bolts, for screwing down the packing. L L, a place for oil or water to keep the air from passing by the piston. M, a pin to take out, to let water through the piston into the pump-barrel to displace the air. N, a ball, that is lifted and let down by a small chain made fast to the pin O. This ball is kept off from its seat by the small chain fastened to the pin O, so that all gravel and sand shall fall into the cistern Q; and when the cistern Q is full, the ball N is let down, and the valve R is lifted up by the line at S, through which the gravel or sand or any other rubbish will fall out. The valve R then must be let down, and the ball N lifted. But this may be done in many other ways. The barrel D may be round, or square, or oval, or any figure that is thought proper, as well as the piston A, which also may be made solid. The water may be delivered through valves at T T, and the same feed-pipe will supply two pumps, as in the common way. The water may be delivered at W, or at what height in the main it will be necessary.

Fig. 9, a double pump on the same principle, but no valves or pipes shewn. Each of these pumps will make their vacuum according to the size of the piston and length of the stroke.

In witness whereof, &c.

Specification

Specification of the Patent granted to ARTHUR WOOLF, *of Wood-street, Spa-fields, in the County of Middlesex, Engineer; for certain Improvements in Steam-Engines.*

Dated July 2, 1805.

TO all to whom these presents shall come, &c. NOW KNOW YE, that in compliance with the said proviso, I the said Arthur Woolf do hereby describe and ascertain the nature of my said invention, and the manner in which the same is to be performed, as follows; that is to say: I have found out and invented a contrivance by which the temperature of the steam-vessel, or working cylinder of a steam-engine, or of the steam vessels or cylinders where more than one are used, may be raised to any required temperature, without admitting steam from the boiler into any surrounding receptacle, whether known by the name of a steam-case or any other denomination; that is to say, instead of admitting steam of a high temperature into such receptacle or steam-case, which is always attended with a risque of explosion, proportioned to the elasticity of the steam employed, I put into the said surrounding receptacle or case, oil or the fat of animals, or wax, or other substances capable of being melted by a lower temperature than the heat intended to be employed, and of bearing that heat without being converted into vapour. Or I put into the said case or cases mercury, or mixtures of metals, as of tin, bismuth, and lead, capable of being kept in a state of fusion in a lower temperature than that intended to be employed in working the steam-engine; and I so form the surrounding case or cases as to make it or them admit the aforesaid oil, or other substance employed, to come in contact, not only with the sides of the steam-vessel or vessels

vessels, or working-cylinder or cylinders, but also with the bottom and top of the same, so that the whole may be as much as possible maintained at one uniform temperature, and this temperature I keep up by a fire immediately under or round the case or cases that contain the aforesaid oil, or other substance, or by connecting the said case or cases with a separate vessel or vessels kept at a proper temperature filled with the oil or other substance made use of as aforesaid. In some circumstances, or whenever the same may be convenient or desirable, I employ the fluid metals, or mixtures of metals and oil, or other of the substances before enumerated, at one and the same time in the same engine; that is to say, in the part of the case or vessel exposed to the greatest action of the fire, I sometimes have the aforesaid metals, or mixtures of metals; and in the parts less exposed to the action of the fire, I put oil, or other substances capable of bearing the requisite heat without being converted into vapour. By this arrangement and method of applying the surrounding heat, I not only obviate the necessity of employing steam of a great expansive force round the steam-vessel or vessels, or the working-cylinder or cylinders, as already mentioned, to maintain them at the temperature required; but I am enabled to obtain from steam, of a comparatively low temperature, or even from water itself, admitted into the steam-vessel or vessels, all the effects that can be obtained from steam of a high temperature, without any of the risque with which the production of the latter is accompanied; not only to the boiler, and other parts of the machinery, but even to the lives of the workmen. For such low steam or even water (but in every case steam is preferable) being admitted into the steam-vessel or vessels, or working-cylinder or cylinders, kept at the requisite higher

temperature

temperature by the afore-mentioned means, will there be expanded in any ratio required, and produce an effect in the working of the engine, which cannot otherwise be obtained but at a greater expense of fuel, or with the risque of an explosion. By this means I can make use of steam, expanded in any required ratio, or of any given temperature, without the necessity of ever having the steam of any greater elasticity than equal to the pressure of the common atmosphere.

Another improvement, which I make use of in steam-engines, consists in a method of preventing, as much as possible, the passage of any of the steam from that side of the piston which is acted upon by the said steam to the other side which is open to the condenser; and this I effect in those steam-engines known by the name of double engines, by employing upon, or about the piston, mercury, or fluid metal or metals, in an altitude equal to the pressure of the steam. The efficacy of this arrangement will appear obvious from attending to what must take place in working such a piston. When the piston is ascending, that is, when the steam is admitted below the piston, the space on its other side being open to the condenser, the steam endeavouring to pass up the side of the piston, is met, and effectually prevented by the column of metal equal or superior to it in pressure, and during the down stroke no steam can possibly pass without first forcing all the metal through. In working what is called a single engine, a less considerable altitude of metal is required, because the steam always acts on the upper side of the piston. For single engines, oil or wax, or fat of animals, or similar substances, in sufficient quantities, will answer the purpose, if another improvement, which constitutes part of my said invention, be applied to the engine; namely, to take care that in either the double

double or single engine so to be worked, the outlet that conveys the steam to the condenser, shall be so posited, and of such a size, that the steam may pass without forcing before it, or carrying with it, any of the metal or other substance employed that may have passed by the piston; taking care at the same time to provide another exit for the metal or other substance collected at the bottom of the steam-vessel or working-cylinder, to convey the same into a reservoir, kept at a proper heat, whence it is to be conveyed to the upper side of the piston by a small pump, worked by the engine, or by any other contrivance. In order that the fluid metal or metals used with the piston may not be oxydated, I always keep some oil, or other proper fluid substance, on its surface, to prevent its coming in contact with the atmosphere; and to prevent the necessity of employing a large quantity of fluid metal, I generally make my piston of the depth of the column required, but of a diameter a little less than the steam-vessel or working-cylinder, excepting where the packing or other fitting is necessary to be applied; so that, in fact, the column of fluid metal forms only a thin body round the piston. In some cases, I make a hollow metallic piston, and apply an altitude of fluid metal in the inside of the same, to press its outside into contact with the steam-vessel or working-cylinder. It may be necessary, however, to state, that in applying my improved method of keeping the steam-vessels of steam-engines at any required temperature to the engine known by the name of Savary's, in any of its improved forms, in which a separate condenser has been introduced, I sometimes employ oil (or any other substance lighter than water, and capable of being kept fluid in the temperature employed without being converted into vapour) in the upper part of the tube or pipe attached to

the steam-vessel; by which means steam of any temperature may be used without being exposed to the risque of partial condensation by the admission of any colder body into the steam-vessel, for the oil or other substance employed for this purpose soon acquires the requisite temperature; and, to prevent unnecessary escape of heat, I construct of, or line with, an imperfect conductor of heat, that part of the tube or pipe attached to the steam-vessel, which may not be heated exteriorly. And farther, (as is already the practice in some engines, and therefore not exclusively claimed by me,) cause the water raised by the engine to pass off through another ascending tube than the one attached to the steam-vessel; but connected with it at some part lower than the oil, or other substance employed in it, is ever suffered to descend to, in the working of the engine.

The improvement which I have just mentioned of introducing oil into the pipe attached to the steam-vessel of such engines, may also be introduced without applying heat externally to the steam-vessel, but in this case part of the effect which would otherwise be gained is lost.

In witness whereof, &c.

Specification of the Patent granted to BARKER CHIFNEY, *of the City of London, Gentleman; for Improvements in the manufacturing and preparing Roofing-Slates, and in laying the same.* Dated March 8, 1803.

TO all to whom these presents shall come, &c. NOW KNOW YE, that in compliance with the said proviso, I the said Barker Chifney do hereby declare, that my said improvements are described in manner following; that is to say: I take the blocks or slabs of slate intended for

for diamond-slating, from the common wet saws in the mill, or elsewhere, after having cut their ends or sides parallel, and then I split them to such thicknesses as may be convenient or necessary. I then put the said slates so split to convenient or necessary thicknesses, and intended for diamond-slating, on a moving table, when, by two toothed circular saws on one spindle, I cut the other two sides or ends at right angles to the ends or sides, cut by the common wet saws, with a motion given by common machinery or otherwise. I afterwards cut off two corners, opposite to each other, so as to give such lap to the slates as the situations of buildings or other circumstances may make necessary, and drill the holes for the screws or nails farther or nearer from the upper point of each slate, according to circumstances.

For laying diamond-slates, when rimerheaded are not used, I bevil the corners cut off, or else file or rasp grooves to admit the heads of the nails or screws, which I secure with putty, or whatever may appear most proper.

To make what I call imperial or common slates, I use various operations, according to the circumstance of the quarries, or such other incidental circumstances as may exist or occur. For instance, I sometimes cut the blocks or slabs to their proper lengths by common wet saws, with common machinery or otherwise, split them afterwards to their necessary thicknesses, and then chop to such breadths as may be convenient or necessary. At other times the blocks or slabs are first split in the common or any other way, to such thicknesses as may be thought convenient or necessary; after which I have the slabs or slates (so split to thicknesses) chopped as near parallel as may be, and to such breadths as may be considered convenient or necessary, when I place each slab or slate on a

moving table, and by two toothed circular saws on one spindle I cut them to their proper lengths with a motion given by common machinery, or otherwise. I also prepare slates in the usual way, or take those prepared by others, and cut them, by common machinery, or otherwise, to convenient or necessary lengths.

To complete the imperial or common slates made or prepared in any of these ways, and make them ready for laying, I drill holes in them with common drills, and by common machinery, or otherwise, farther from or nearer to their side edges, and farther from or nearer to their top edges, according as the purposes for which they are intended, or other circumstances may suggest. It is not absolutely necessary that the top or upper ends of imperial or common slates should be square, round, or of any other determinate shape, but I approve and recommend them to be square, for the advantage of pointing in the inside. The heads of the nails with which imperial or common slates are fastened to the boards or batten-laths, are to be secured with putty, or otherwise, as may be thought necessary.

There is also another method in which I prepare what I call imperial or common slates for laying, whether cut at their ends to proper lengths, split to their necessary thicknesses, and reduced to their proper breadths, and cut to their proper lengths, or made in the usual way, and cut to their proper lengths; that is to say, by cutting notches, or indents at their sides with saws, with files, or with rasps, by common machinery, or otherwise, so as to admit sufficient hold to the nails, and to be in lieu of the holes before mentioned.

In witness whereof, &c.

Specification

Specification of the Patent granted to JOHN GREGORY HANCOCK, *of Birmingham, in the County of Warwick, Die-Engraver; for a Method of forcing or working the Bolts of Presses or of Engines used for the Purpose of cutting, pressing, and squeezing, of Metals, Horn, Tortoise-shell, Leather, Paper, and other Substances.*

Dated September 14, 1804.

With a Plate.

TO all to whom these presents shall come, &c. NOW KNOW YE, that I the said John Gregory Hancock, in compliance with, and in performance of, the aforesaid proviso, in the said in part recited letters patent contained, do, by this present instrument in writing, declare that my said invention is herein particularly described. Whereas the bolts of presses for cutting out, stamping, and impressing metals, are commonly forced or worked by the application of screws, which necessarily have the same alternate advancing and retrograde motion which is required of the bolts; now, in order to force or work the said bolts by a power which has a continued rotary motion, as well as by a power which has an alternate advancing and returning or vibratory motion, instead of a screw I use a shaft, spindle, or axle, to one or both ends of which, or to the circumference, or some other part, whereof I affix a projection, which I call an inclined plane, curve, or wedge; or else I so form and shape the shaft, spindle, or axle, that, by its rotation, it will itself act as an inclined plane, curve, or wedge, and press upon the bolt, as the thicker or more projecting part of the inclined plane, curve, or wedge, advances. The said inclined plane, curve, or wedge, may be of any angle or curve, not making an angle of more than thirty degrees, or thereabouts, with the base line. Sections of those I generally use are represented in the drawing,

Figs.

Figs. 1, 2, 3, and 4; any of which may be used when a continued rotary motion is given to the shaft, spindle, or axle; but if an alternate advancing and returning or vibratory motion be given to the shaft, spindle, or axle, then I make use of the double inclined plane, curve, or wedge, Figs. 3 and 4. The end, or part of the bolt upon which the inclined plane, curve, or wedge works, may be either flat, or of any prominent angle or curve. In general I make the end of the bolt a little convex, or else I affix a roller into the bolt, upon which the inclined plane, curve, or wedge works, and which I find considerably diminishes the friction. The said inclined plane, curve, or wedge, and bolt, may be made of such metal, or other hard substance, as may be found most economical, and best adapted to the purpose to which the machine is applied; but in general I make use of hardened steel. The shaft, spindle, or axle, on which the said inclined plane, curve, or wedge, is fixed, and the bolt, must be so placed that, when a rotary or an alternate advancing and returning or vibratory motion is given to the shaft, spindle, or axle, the inclined plane, curve, or wedge, will act upon the bolt, and force it along, depress, or elevate it in its case, box, or groove. In general I so place the shaft, spindle, or axle, and bolt, that the base, or part of the inclined plane, curve, or wedge, which is in contact with the shaft, spindle, or axle, shall be at a right angle, or nearly so, with the bolt, see Figs. 8, 9, and 10. The shaft, spindle, or axle, and the case, box, and groove, in which the bolt works, should be firmly fixed, so that their position with respect to each other cannot vary. For this purpose I recommend that the case, box, or groove, in which the bolt works, should be fixed to the frame of the machine; and if the inclined plane, curve, or wedge, be fixed on the

the ends of the shaft, spindle, or axle, I recommend that the shaft, spindle, or axle, should work in a collar in the frame of the machine, and have a projection or shoulder that will rest against the collar, as represented in the drawing, Figs. 7 and 10; or otherwise, that the end of the shaft, spindle, or axle, farthest from the bolts, should rest or butt against some firm and stationary part of the machine, or the frame on which it is placed, as represented in the drawing, Fig. 8. But if the inclined plane, curve, or wedge, be fixed on the circumference of the shaft, spindle, or axle, then I recommend that the shaft, spindle, or axle, should run in the two grooves or brasses in the frame of the machine, as represented in the drawing Fig. 9. The shaft, spindle, or axle, on which the inclined plane, curve, or wedge, is fixed, and the bolt, with the case, box, or groove, in which the bolt works, being placed in the situation before described, a rotary or vibrating motion is given to the shaft, spindle, or axle, which brings the inclined plane, curve, or wedge, into action; and as the motion is continued, the inclined plane, curve, or wedge, by pressing against the bolt, or the roller placed thereon, forces the bolt along its case, box, or groove. In order to force the bolt back, I make use of a spring, or springs, which are affixed in the manner represented in the drawing, Figs. 8, 9, and 10, or in any other manner that may be more convenient; or the bolt may be forced back by a lever and weight, or by any other means. The bolt and shaft, spindle, or axle, may be placed in a perpendicular or inclined position, as may be found best adapted to the purpose to which the machine is applied. But if the shaft, spindle, or axle, be placed in a perpendicular or inclined position, I recommend that a pin, pivot, or gudgeon, should project from the centre of the lower end thereof,

of

of such a length that it will rest upon the case or box in which, or the back against which, the bolt works, or against some other part of the machine, so as to support the weight of the shaft, spindle, or axle, see drawing, Figs. 7 and 10. I use one or both ends, or any other part, of such shafts, spindles, or axles, and affix one or more inclined planes, curves, or wedges, thereto; and I apply my machines to the purposes of cutting, pressing, stamping, impressing, and squeezing, of metals, horn, tortoiseshell, leather, paper, and other substances: sometimes I fix the inclined plane, curve, or wedge, upon the end of the bolt, and place a roller in, and projecting from, the shaft, spindle, or axle. I also sometimes place a lever, centered at one end, over the end of the bolt, and apply the inclined plane, curve, or wedge, to the extremity or moveable end of the lever. But I do not find either of these methods so good as the method before described.

EXPLANATION of DRAWINGS.

Fig. 1, (Plate V.) Section of inclined plane, curve, or wedge, to be fixed on the circumference of a shaft, spindle, or axle, to which a continued rotary motion is given.

Fig. 2. Section of inclined plane, curve, or wedge, to be fixed on the end of a shaft, spindle, or axle, to which a continued rotary motion is given.

Fig. 3. Section of double inclined plane, curve, or wedge, to be fixed on the end of a shaft, spindle, or axle, to which an alternate advancing and returning or vibratory motion is given.

Fig. 4. Section of double inclined plane, curve, or wedge, to be fixed on the circumference of a shaft, spindle, or axle, to which an alternate advancing and returning or vibratory motion is given.

Fig.

Fig. 1.

Fig. 3.

Fig. 4.

Fig. 10.

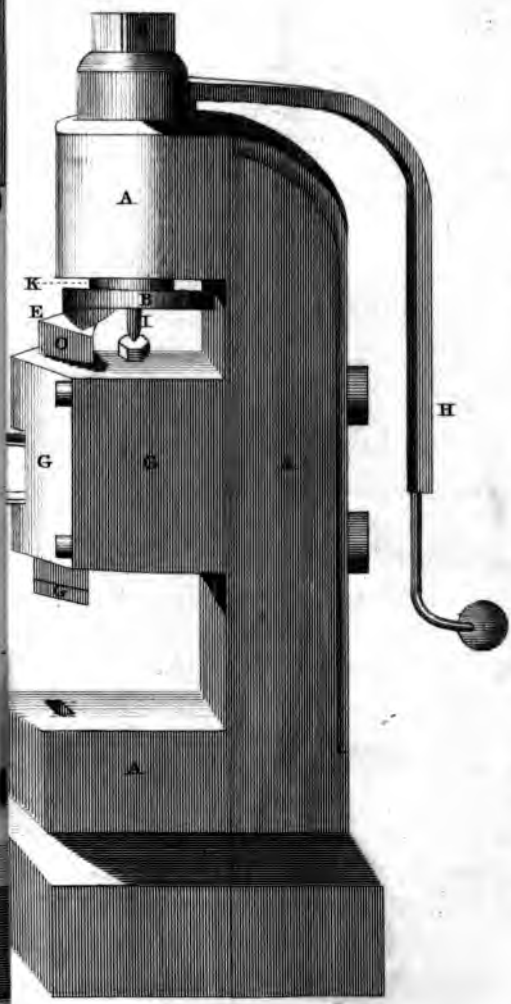


Fig. 8.

Fig. 5. Section of shaft, spindle, or axle, with three inclined planes, curves, or wedges, affixed to the circumference thereof.

Fig. 6. End of shaft, spindle, or axle, with three inclined planes, curves, or wedges, affixed thereto.

Fig. 7. Shaft, spindle, or axle, of machine, Fig. 10.

Fig. 8. Machine in which the shaft, spindle, or axle, (with an inclined plane, curve, or wedge, on the end thereof,) and the bolt, are placed in an horizontal position.

Fig. 9. Machine in which the shaft, spindle, or axle (with an inclined plane, curve, or wedge, on the circumference thereof,) is placed in a horizontal position, and the bolt in a perpendicular position.

Fig. 10. Machine in which the shaft, spindle, or axle, (with a double inclined plane, curve, or wedge, on the end thereof,) and the bolt, are placed in a perpendicular position. This machine is intended to be worked by a vibratory motion, but it may also be worked by a continued rotary motion.

In Figs. 7, 8, 9, and 10, A, represents the frame of the machine. B, represents the shaft, spindle, or axle. C, the bolt. E, the inclined plane, curve, or wedge. F, the springs. In Fig. 8 there are two springs, one on each side, affixed to the frame of the machine, and acting upon two pins, which project from the sides of the bolt. In Figs. 9 and 10 one end of the spring rests against the case of the bolt, and the other end against a notch in the bolt itself. G, the case, box, or groove, in which the bolt works, a part of which is cut away, to enable the spring to act. H, the handle or wheel to which the power is applied. I, the pin, to support the weight of the shaft, spindle, or axle, in Fig. 10. K, the shoulder, on shaft, spindle, or axle, which rests against the collar in the frame of the machine, Fig. 10.

In witness whereof, &c.

On the Reproduction of Buds. By THOMAS ANDREW KNIGHT, *Esq. F. R. S. In a Letter to the Right Hon. Sir* JOSEPH BANKS, *K. B. P. R. S.*

From the PHILOSOPHICAL TRANSACTIONS of the ROYAL SOCIETY.

EVERY tree in the ordinary course of its growth generates, in each season, those buds which expand in the succeeding spring; and the buds thus generated, contain, in many instances, the whole of the leaves which appear in the following summer. But if these buds be destroyed during the winter, or early part of the spring, other buds, in many species of trees, are generated, which in every respect perform the office of those which previously existed, except that they never afford fruit or blossoms. This reproduction of buds has not escaped the notice of naturalists; but it does not appear to have been ascertained by them from which, amongst the various substances of the tree, the buds derive their origin.

Du Hamel conceived that reproduced buds sprang from preorganized germs: but the existence of such germs has not, in any instance, been proved, and it is well known that the roots, and trunk, and branches, of many species of trees will, under proper management, afford buds from every part of their surfaces; and therefore, if this hypothesis be well founded, many millions of such germs must be annually generated in every large tree; not one of which, in the ordinary course of nature, will come into action: and as Nature, amidst all its exuberance, does not abound in useless productions, the opinions of this illustrious physiologist are, in this case, probably erroneous.

Other

Other naturalists have supposed the buds, when reproduced, to spring from the plexus of vessels which constitutes the internal bark; and this opinion is, I believe, much entertained by modern botanists: it nevertheless appears to be unfounded, as the facts I shall proceed to state will evince.

If the fruit-stalks of the sea cale *(cambe maritima)* be cut off near the ground in the spring, the medullary substance, within that part of the stalk which remains attached to the root, decays; and a cup is thus formed in which water collects in the succeeding winter. The sides of this cup consist of a woody substance, which in its texture and office, and mode of generation, agrees perfectly with the alburnum of trees; and I conceive it to be as perfect alburnum as the white wood of the oak or elm: and from the interior part of this substance, within the cup, I have frequently observed new buds to be generated in the ensuing spring. It is sufficiently obvious, that the buds in this case do not spring from the bark; but it is not equally evident that they might not have sprung from some remains of the medulla.

In the autumn of 1802 I discovered that the potatoe possessed a similar power of reproducing its bud. Some plants of this species had been set, rather late in the preceding spring, in very dry ground, where, through want of moisture, they vegetated very feebly; and the portions of the old roots remained sound and entire till the succeeding autumn. Being then moistened by rain, many small tubers were generated on the surface made by the knife in dividing the roots into cuttings; and the buds of these, in many instances, elongated into runners, which gave existence to other tubers, some of which I had the pleasure to send to you.

I have in a former paper remarked, that the potatoe consists of four distinct substances, the epidermis, the true skin, the bark, and its internal substance, which, from its mode of formation, and subsequent office, I have supposed to be alburnous: there is also in the young tubes a transparent line through the centre, which is probably its medulla. The buds and runners sprang from the substance which I conceive to be the alburnum of the root, and neither from the central part of it, nor from the surface in contact with the bark. It must, however, be admitted, that the internal substance of the potatoe corresponds more nearly with our ideas of a medullary than of an alburnous substance; and therefore this, with the preceding facts, is adduced to prove only that the reproduced buds of these plants are not generated by the cortical substance of the root: and I shall proceed to relate some experiments on the apple, and pear, and plum-tree, which I conceive to prove that the reproduced buds of those plants do not spring from the medulla.

Having raised from seeds a very considerable number of plants of each of these species in 1802, I partly disengaged them from the soil in the autumn, by digging round each plant, which was then raised about two inches above its former level. A part of the mould was then removed, and the plants were cut off about an inch below the points where the seed-leaves formerly grew; and a portion of the root, about an inch long, without any bud upon it, remained exposed to the air and light. In the beginning of April I observed many small elevated points on the bark of these roots, and, removing the whole of the cortical substance, I found that the elevations were occasioned by small protuberances on the surfaces of the alburnum.

alburnum. As the spring advanced, many minute red points appeared to perforate the bark: these soon assumed the character of buds, and produced shoots in every respect similar to those which would have sprung from the organized buds of the preceding year. Whether the buds thus reproduced derived any portion of their component parts from the bark or not, I shall not venture to decide; but I am much disposed to believe that, like those of the potatoe, they sprang from the alburnous substance solely.

The space, however, in the annual root, between the medulla and the bark is very small; and therefore it may be contended that the buds in these instances may have originated from the medulla. I therefore thought it necessary to repeat similar experiments on the roots and trunks of old trees, and by these the buds were reproduced precisely in the same manner as the annual roots: and therefore, conceiving myself to have proved in a former memoir *, that the substance which has been called the medullary process does not originate from the medulla, I must conclude, that reproduced buds do not spring from that substance.

I have remarked in a paper, which you did me the honour to lay before the Royal Society in the commencement of the present year, that the alburnous tubes at their termination upwards invariably join the central vessels, and that these vessels, which appear to derive their origin from the alburnous tubes, convey nutriment, and probably give existence, to new buds and leaves. It is also evident, from the facility with which the rising sap is transferred from one side of a wounded tree to the

* Phil. Trans. of 1803.

other,

other, that the alburnous tubes possess lateral, as well as terminal, orifices: and it does not appear improbable that the lateral as well as the terminal orifices of the alburnous tubes may possess the power to generate central vessels; which vessels evidently feed, if they do not give existence to, the reproduced buds and leaves. And therefore, as the preceding experiments appear to prove that the buds neither spring from the medulla nor the bark, I am much inclined to believe that they are generated by central vessels which spring from the lateral orifices of the alburnous tubes. The practicability of propagating some plants from their leaves may seem to stand in opposition to this hypothesis; but the central vessel is always a component part of the leaf, and from it the bud and young plant probably originate.

I expected to discover in seeds a similar power to regenerate their buds; for the cotyledons of these, though dissimilar in organization, execute the office of the alburnum, and contain a similar reservoir of nutriment, and at once supply the place of the alburnum and the leaf. But no experiments, which I have yet been able to make, have been decisive, owing to the difficulty of ascertaining the number of buds previously existing within the seed. Few, if any, seeds, I have reason to believe, contain less than three buds, one only of which, except in cases of accident, germinates; and some seeds appear to contain a much greater number. The seed of the peach appears to be provided with ten or twelve leaves, each of which probably covers the rudiment of a bud, and the seeds, like the buds of the horse-chesnut, contain all the leaves, and apparently all the buds of the succeeding year: and I have never been able to satisfy myself that all the buds were eradicated without having

having destroyed the base of the plumule, in which the power of reproducing buds probably resides, if such power exists.

Nature appears to have denied to annual and biennial plants (at least to those which have been the subjects of my experiments) the power which it has given to perennial plants to reproduce their buds; but nevertheless some biennials possess, under peculiar circumstances, a very singular resource, when all their buds have been destroyed. A turnip, bred between the English and Swedish variety, from which I had cut off the greater part of its fruit-stalks, and of which all the buds had been destroyed, remained some weeks in an apparently dormant state; after which the first seed in each pod germinated, and bursting the seed-vessel, seemed to execute the office of a bud and leaves to the parent plant, during the short remaining term of its existence, when its preternatural foliage perished with it. Whether this property be possessed by other biennial plants in common with the turnip or not, I am not at present in possession of facts to decide, not having made precisely the same experiment on any other plant.

I will take this opportunity to correct an inference that I have drawn in a former paper *, which the facts (though quite correctly stated) do not, on subsequent repetition of the experiment, appear to justify. I have stated, that when a perpendicular shoot of the vine was inverted to a depending position, and a portion of its bark between two circular incisions round the stem removed, much more new wood was generated on the lower lip of the wound become uppermost by the inverted po-

* Phil. Trans. of 1803.

sition

sition of the branch, than on the opposite lip, which would not have happened had the branch continued to grow erect; and I have inferred that this effect was produced by sap which had descended by gravitation from the leaves above. But the branch was, as I have there stated, employed as a layer, and the matter which would have accumulated on the opposite lip of the wound had been employed in the formation of roots, a circumstance which at that time escaped my attention. The effects of gravitation on the motion of the descending sap, and consequent growth of plants, are, I am well satisfied, from a great variety of experiments, very great; but it will be very difficult to discover any method by which the extent of its operation can be accurately ascertained. For the vessels which convey and impel * the true sap, or fluid from which the new wood appears to be generated, pass immediately from the leaf-stalk towards the root; and though the motion of this fluid may be impeded by gravitation, and it be even again returned into the leaf, no portion of it, unless it had been extravasated, could have descended to the part from which the bark was taken off in the experiment I have described. I am not sensible that in the different papers which I have had the honour to address to you, I have drawn any other inference which the facts, on repetition of the experiments, do not appear capable of supporting.

* Phil. Trans. of 1804.

Description

Description of a Property of Caoutchouc, or Indian Rubber; with some Reflections on the Cause of the Elasticity of this Substance. By Mr. JOHN GOUGH.

From the MEMOIRS of the LITERARY and PHILOSOPHICAL SOCIETY of MANCHESTER.

THE substance called Caoutchouc, or Indian Rubber, possesses a singular property, which, I believe, has never been taken notice of in print, at least by any English writer; the present letter contains my experiments and reflections on the subject; and should they appear to deserve the attention of your philosophical friends, I am certain you will take the trouble of communicating the paper to the Literary and Philosophical Society of Manchester.

The property I am about to describe depends on the temperature of the Caoutchouc which is used in the experiment; for heat increases the pliancy of the substance, and cold, on the contrary, renders it more rigid; so that when a slip of this resin has been sufficiently warmed, it may be extended to more than twice its natural length, by a moderate force applied to its extremities, after which it will recover its original dimensions in a moment, provided one of the ends of it be let go as soon as it has been stretched. This disposition of the substance may be produced by a degree of temperature less than the heat of the blood; it is therefore necessary to prepare a slip of it, by steeping it for a few minutes in warm water, or by holding it somewhat longer in the fist; either of these precautions makes the resin pliant, and fits it for the experiment, which is performed in the following manner.

I made a piece of Caoutchouc a little heavier than an equal bulk of water, the temperature of which was 45 degrees: the vessel containing the resin and water was then placed on the fire; and when the contents of it were heated to 130 degrees, the Caoutchouc floated on the surface.

Experiment I.

Hold one end of the slip, thus prepared, between the thumb and fore-finger of each hand; bring the middle of the piece into slight contact with the edges of the lips; taking care to keep it straight at the time, but not to stretch it much beyond its natural length: after taking these preparatory steps, extend the slip suddenly; and you will immediately perceive a sensation of warmth in that part of the mouth which touches it, arising from an augmentation of temperature in the Caoutchouc; for this resin evidently grows warmer the farther it is extended, and the edges of the lips possess a high degree of sensibility, which enables them to discover these changes with greater facility than other parts of the body. The increase of temperature, which is perceived upon extending a piece of Caoutchouc, may be destroyed in an instant, by permitting the slip to contract again, which it will do quickly, by virtue of its own spring, as oft as the stretching force ceases to act, as soon as it has been fully exerted. Perhaps it will be said, that the preceding experiment is conducted in a negligent manner; that a person, who wishes for accuracy, will not trust his own sense of feeling in inquiries of this description, but will contrive to employ a thermometer in the business. Should the objection be started, the answer to it is obvious; for the experiment in its present state demonstrates the reality of a singular fact; by convincing that sense, which is the only direct judge in the case, that the temperature of a

piece

piece of Caoutchouc may be changed, by compelling it to change its dimensions. The use of a thermometer determines the relative magnitudes of these variations, by referring the question of temperature to the eye; experiments of this sort are therefore of a mathematical nature, and afford a kind of knowledge with which we have nothing to do at present; for we are not inquiring after proportions, but endeavouring to establish the certainty of a fact, which may assist in discovering the reason of the uncommon elasticity observable in Caoutchouc. My essay or letter appears to be running into a long digression; the subject must therefore be resumed, and it will not be improper to premise the following simple experiment in the present state of the inquiry; because it seems capable of affording no inconsiderable degree of insight into the plan which nature pursues in producing the phenomenon in question.

Experiment II.

If one end of a slip of Caoutchouc be fastened to a rod of metal or wood, and a weight be fixed to the other extremity, in order to keep it in a vertical position, the thong will be found to become shorter with heat and longer with cold. The processes of heating, cooling, and measuring bodies are so well known, that I need not enter into the minuter parts of the experiment; it will be proper however to add, that an increase of temperature diminishes the specific gravity of the Indian Rubber, and a loss of heat occasions a contrary effect in it, as I have proved experimentally. The knowledge of the latter fact leads me to conclude, apparently on reasonable grounds, that the pores or interstices of Caoutchouc are enlarged by heat, and diminished by cold; consequently when a slip of this substance, which remains extended by

a weight, or the application of force, happens to contract from an accession of temperature, the capacity of its pores, taken separately or collectively, is augmented by the change that takes place in the figure of the thong. Now, if the existence of caloric be admitted, it will follow, from the preceding arguments, that the phenomenon under consideration is occasioned by the alternate absorption and emission of the calorific fluid, in the same manner that ropes, the blades of Fuci, as well as many more bodies, are obliged to contract and extend themselves, by the alternate absorption and emission of water. You will perceive, by the tenour of the foregoing observations, that my theory of this case of elasticity is perfectly mechanical; in fact, the explanation of it depends upon the mutual attraction of caloric and Caoutchouc; the former of which penetrates the latter, and pervades every part of it with the greatest ease and expedition; by which the resin is compelled to accommodate its pores to that portion of the calorific fluid which is due to its whole mass, at any particular degree of temperature. In order to apply the last remark to the phenomenon under consideration I may observe, that if a force be exerted on a piece of Caoutchouc to alter the dimensions of its pores, the mutual attraction mentioned above will resist the effort. But the ease with which this substance may be made to change its figure, and the retractile power which it possesses on these occasions, shew that its constituent particles move freely amongst themselves: but where there is motion there is void space; consequently Caoutchouc abounds with innumerable pores or interstices, the magnitudes of which are variable, because the specific gravity of the resin becomes less with heat and greater with cold. Now if the dimensions of the pores in a piece of Caoutchouc can be lessened, without taking

taking away part of the matter of heat which it contains at the time, this new arrangement in the internal structure of the slip will lessen its capacity for the matter of heat, and consequently augment its temperature. But the warmth of such a slip is increased by stretching it, according to the first experiment; the pores of it are therefore diminished, and the effort which it exerts at the time arises from the mutual attraction of the Caoutchouc and caloric; which attraction causes an endeavour to enlarge the interstices of the former for the reception of the latter; hence it happens, that the thong contracts longitudinally, according to the second experiment, and the redundant caloric is absorbed in the course of this operation, which again reduces the temperature. The preceding explanation agrees very well with the phenomenon, as it is stated in the beginning of this letter; and the theory receives additional confirmation from the following facts.

Experiment III.

If a thong of Caoutchouc be stretched in water warmer than itself, it retains its elasticity unimpaired; on the contrary, if the experiment be made in water colder than itself it loses part of its retractile power, being unable to recover its former figure; but let the thong be placed in hot water, while it remains extended for want of spring, and the heat will immediately make it contract briskly. The foregoing circumstances may be considered as proving, that the elasticity of Caoutchouc is not a constitutional quality of the substance, but a contingent effect arising from the loss of equilibrium between the portion of caloric, which the resin happens to contain at any moment, and its capacity to receive that fluid at the same instant. The object of the present letter is to demonstrate,

monstrate, that the faculty of this body to absorb the calorific principle may be lessened by forcibly diminishing the magnitudes of its pores, and this essential point of the theory may be confirmed by experiment; for the specific gravity of a slip of Caoutchouc is increased by keeping it extended while it is weighed in water.

Observations on Mr. HAWKINS's *Patent for Improvements on Musical Instruments. Communicated by the Inventor; in a Letter to the Editors.*

GENTLEMEN,

AS you have thought proper to introduce in your last number the specification of the patent obtained by my father when I was in America, for my improvements on musical instruments, &c. I must ask permission to state, through the medium of your useful work, what parts of the described improvements are at present ready for public inspection, in order that your readers may not be disappointed in supposing the whole of them are already completed.

The inelegant appearance of the common grand piano forte has become tolerable only in consequence of the delightful music it produces; a more uniform and handsome shape, as well as a more portable and convenient size, has long been wished. It has also been a source of great complaint, that piano fortes of the common construction are put out of tune by every change in the atmosphere.

To remedy these, and several other defects, I have combined the first, third, fifth, eighth, tenth, and eleventh articles of the specification, and constructed grand piano fortes

fortes on which the weather will not have any effect to put them out of tune, which are of a uniform and elegant shape, and occupy less than one-fourth of the room taken up by the common kind.

The spiral elastic wire-strings described in the first article are the means of contracting the size of the instrument; a spiral string, extending but three feet in length, producing the same bass tone as a straight one six feet long; the great obstacle to making piano fortes in a small space, hitherto having been the obligation of using long strings to produce the bass.

Many persons on first seeing these spiral bass strings imagine they will easily be pulled straight; but that this cannot take place may be known, when it is considered, that being in themselves springs, they possess a constant tendency to contraction; so that the exertion to recover their original compass continually counteracts the force by which they are extended.

The position of the strings, as in article the third, can be adopted only where the bass are spiral, since all others require great length for the bass; and if long strings are used perpendicularly, they must of necessity extend from about the height of the finger-keys, towards the cieling, as in the common upright instruments, which are by no means portable.

The tuning apparatus and metal frame, described in the fifth article, prevent the effects of the atmosphere on the instrument. When strings are stretched on a frame of wood, as in the usual way, and tuned by pins driven into the wood, it is not possible but their tension must be altered by every variation of the atmosphere; for should the instrument be tuned when the weather is cold, a succeeding warm air will immediately expand the strings, and thereby lessen their tension; and if it be then tuned

again,

again, the opposite effect will take place on the return of cold; for it is a known law of nature, that metals are expanded by heat and contracted by cold. It is also well known, that moisture causes wood to swell, and dryness makes it shrink; so that if an instrument of the common construction be tuned when the air is cold and moist, a double loss of tension will be the consequence of a change to warm and dry, and *vice versâ*. All these inconveniences are prevented by my metal frames; for if the heat expand, or cold contract the strings, the frame on which they are stretched will be expanded or contracted in like manner, and consequently a uniform tension always kept up: experience fully confirms this fact.

By exposing the sound-board on both sides, as in the eighth article, greater fulness and clearness of tone is obtained.

It is customary in grand piano fortes, to move the whole of the finger-keys and hammers, so as occasionally to strike only one of the three strings of each note, in order to produce very soft tones. One bad consequence of this is, that the force of the hammer being expended on that string, it must of necessity give way; and it is not uncommon, from this circumstance, for an instrument to be rendered entirely discordant in a few minutes after it has been tuned. I have obtained a much more delightful softness without any risk of hurting the tune of the instrument, by interposing pieces of soft leather between the hammers and strings, as described in article the tenth; these pieces of leather are also cut like a wedge, so that the effect of a swell is produced by gradually passing the leather from the thick to the thin part.

By turning the finger-keys on pivots, as in article the eleventh, much room is gained when the instrument is not in use.

These

These improvements constitute my grand piano forte; but I have also adapted the most essential parts of them to the common square piano, which I am enabled to sell at about the usual prices.

The above are all the parts of the patent as yet completed in this country; but I shall shortly bring the second, fourth, eighth, ninth, and eleventh articles before the public in a new musical instrument, which I call the *Claviole*, from its being played on with finger-keys, and comprehending all instruments of the viol kind, from the violin to the double bass. I completed, and publicly exhibited, a claviole, about four years since in America, under the patent which I procured there, at the same time this was taken out by my father. But an instrument on similar principles was announced a few months since in France, by the name of the *Orchestrino*, as a new discovery; whether or no that was taken from mine is perhaps not easy to determine. The one I exhibited was considered, by the first musicians, equal in power to fifteen violins, tenors, and basses, and was at the same time capable of being played soft enough for an accompaniment to the most delicate lady's voice, by the touch of the finger alone.

By rendering the gut-strings of the claviole waterproof, as in article second, the dampness of the atmosphere will have but little effect upon them; but by being stretched with springs (article four) they will keep in tune many months, perhaps years. I stretched forty-five gut strings, of various sizes and lengths, and tuned them with a spring to each, and they remained in good tune during five months without one breaking, or being likely to break.

The claviole has been among the desiderata of musical mechanicians for a length of time; the principal thing

wanted was, an equivalent to the common violin-bow, but possessing the power of rotation. Horse-hair only was admissible, as being the best known substance to draw tone from a gut-string. A circular motion appeared the best calculated to produce a continued sound. But how to apply horse-hair to a wheel, and still to preserve all the elasticity of the violin-bow, has been often sought without success. I was, however, fortunate enough, ten years since, to contrive the method described in article nine, and then made a circular bow, which possessed all the smoothness and elasticity of the violin-bow. This I attached to a violin; and by giving it motion drew the finest tones of which the violin was capable, and which were continued during pleasure; for although the inner surface of the hair is polygonic and not circular, yet the angles are so very obtuse that the finest ear cannot discover their passage over the strings.

The eighth and eleventh articles give me the same advantages in the claviole as in the piano forte.

So soon as the manufacture of the claviole is established, I shall turn my attention to the sixth, seventh, and twelfth articles; and also the ramifications of the articles one, two, and four, unless I should previously dispose of these branches of the patent to persons in the respective lines of business, which I am willing to do on liberal terms.

The application of spiral springs to saddles and stirrup-straps was brought forward with great success in America, immediately on taking out the patent. Great numbers of saddles were made, and purchased with avidity, by the first horsemen, who recommended them to their friends with the highest encomiums, as giving perfect ease to the rider even on the roughest trotting horse.

The

The spiral springs, applied to the suspending of riding-carriages, were also used, and highly approved; their expense is trifling compared with the common coach-springs, and any requisite degree of strength and elasticity can be given by increasing the number and length.

There are other modifications of the principles of this patent which I intend to bring forward as soon as possible, of which the public will be duly informed.

I am, Gentlemen,

Yours, &c.

JOHN ISAAC HAWKINS.

No. 4, Dalby Terrace, Dec. 20, 1805.

Of the Influence of Manganese in the Production of Iron on a large Scale. By M. STUENKEL, *Junior. Translated from the German by M.* DAUBUISSON.

From the JOURNAL DES MINES.

AMONG the great number of writings which we have upon iron, several of which are extremely valuable, there is as yet not one which treats fully of the influence of manganese upon the production of iron and of steel on a large scale. The authors who have written upon the working of iron were either mere theorists, too little acquainted with the manner in which iron is produced in founderies and forges, or else able practitioners, but who have treated only of some particular objects, and confined themselves too much to local circumstances.

The first have presented the public with chemical analyses and reasonings upon the constituent principles of iron, and to them we are indebted for the sound theory which we at present possess respecting iron in general, a

theory which bears a just proportion to the actual state of chemical knowledge. The practical authors, by their instructions and their own labours, have brought certain parts of the forging art to a degree of perfection which, to judge from appearance, leaves little more to be desired.

Neither of them, however, appear to have paid sufficient attention (at least in what concerns the practice of the production and refining of iron) to the difference between the two principal kinds of cast iron, the *white-radiated* produced from the manganesiferous ores, and the *grey-granulated* obtained from the other ores.

Having had occasion to make some observations on this subject, I hope they will be favourably received by professional persons.

The cast-iron produced from ores which contain manganese, differs essentially from that which is obtained from such as contain none. The nature of these two species of cast metal, and the farther working of them, require that attention should be paid to this difference. The authors who have not paid sufficient attention to this circumstance, and who have contented themselves with distinguishing the harsh and brittle cast metal from that which was less so, have taken the denomination of *grey cast metal* for synonimous with *smooth* or good *(gaar* Germ.) cast metal, and that of *white* for *harsh*, bad *(Grell.* Germ.) cast metal. I conceive it more proper to employ the words smooth and harsh *(gaar* and *grell)* in their original signification. The epithet of *white* will designate the cast metal proceeding from the manganesiferous minerals; the other will be called *grey* cast metal.

The *white*-radiated is distinguished as easily from the *grey*-granulated as one metal can be from another; it will

will therefore be easy to recognize them without knowing the manner in which they have been produced.

All the iron ores which contain manganese yield *white cast metal*, in whatever manner their fusion is performed. The number of these ores is small: I am acquainted with but two species, *viz. the spathic iron ore*, and *the brown iron ore*. (*Mineralog. de Werner*, tom. II. p. 257.) They always contain (at least the first does) manganese; whereas this substance is found only rarely and accidentally in the other iron ores. The more there is put of manganesiferous ores into the composition of a casting, the more manganese combines in consequence with the cast metal, and the more this possesses the properties which characterise the *white* cast.

In the forges where manganesiferous ores are not melted, it is impossible to produce a similar cast metal; that is to say, a cast metal possessing all the properties which characterise it, and which we shall detail in the sequel. Hence, and even without chemical analysis being required, we may conclude, that during the fusion of the ores the manganese is reduced at the same time with the iron, and that it is combined with it in the cast metal.

The two species of cast metal, the *white* and the *grey*, pass frequently into one another by insensible gradations; and then we cannot distinguish the first, unless when we know that manganese existed in sufficient quantity in the ores that were fused. A fourth part of spathic iron ore, added to ores which contain no manganese, is sufficient to render the cast metal such as may be ranked among the *white* sort.

It is not indeed altogether impossible to produce in the founderies, with manganesiferous ores, a cast metal, the aspect of which approaches to that of the grey species;

species; that is to say, it shall be of a deep grey colour, and granulated (only of a fine grain); but this cannot happen unless when only some of the ores that are fused are manganesiferous, and when, besides, an excess of charcoal in proportion to the ore is put into the charges. But if only spathic ores and black hæmatites (which is known to be manganesiferous) are fused, it is then absolutely impossible, whatever excess of charcoal may be employed, to obtain a cast metal of a deep grey. In the furnaces which are called *Blauoefen* (used in Styria) a cast metal of a grey colour can never be obtained as soon as only the half of the ores is manganesiferous.

In an elementary treatise on the metallurgical treatment of iron, it would be necessary to make two sections; since the production and the refining of the two species of cast metal of which we speak require processes totally different from each other. The manipulations that are to be practised upon ores containing manganese differ widely from those to be used in treating such as do not contain any.

As the pure manganese ore is rarely to be met with in nature, and, as when furnaces are erected, it is generally done in situations contiguous to iron mines and forests, it will hardly be possible to obtain and to work white cast metal, unless in places where nature has deposited manganesiferous iron ores.

The principal places in Germany where the *white* cast metal is wrought, are Styria, Carinthia, Carniola, the territory of Nassau-Siegen, Smalcalde in Hesse, Maedgesprung and Gettelde in the Hartz, Louisenthal in Saxony, &c. In Silesia, in the Margravate of Brandenburg, in Lusatia, in Bohemia, in the Erzgebirge of Saxony, in the Hara Mountains, (with exception of the forges above mentioned,)

mentioned,) the *grey* cast metal is obtained *. At Magdesprung there is one furnace which yields white and another which yields grey cast metal: the former is charged for the greater part with spathic ore, and the latter with ores that contain no manganese.

In the forges where argillaceous and siliceous ores are fused, they ought to consider themselves very fortunate when they have pure spathic ore, especially if it is decomposed; for then it is very easy to make a very fusible composition: and when the ore is not generally very poor, a large quantity of iron may be obtained in a short time.

The spathic iron ore, which is very fusible by itself, especially when, by an effect of decomposition, it has become brown or black, favours, in a singular manner, the fusion of the other ores, even when it is mixed with them only in a small quantity. In default of spathic ore, they are obliged to employ lime-stone, which renders the composition poorer, and does not equally favour the fusion.

The black hæmatites, especially when it is decomposed, is also advantageous, and even more so than the spathic iron ore: it more easily yields a smooth cast metal, and is very fusible by itself; mixed with the other iron ores, it considerably augments the produce of the melting: it contains, as well as the spathic ore, a large proportion of manganese. These two iron ores mixed together, when they are decomposed, afford the most fusible composition.

* When in these places a cast metal of a white colour is obtained, without manganesiferous ores having been employed, it is because the cast metal is harsh and brittle, and the operation has not been well conducted: this cast metal still belongs to our grey species.

A remark-

A remarkable and very advantageous property of manganese in the fusion of the iron ores, is that of destroying the bad effects of the ponderous spar which very frequently accompanies these ores. Ponderous spar is found mixed with a great number of iron ores; and when these do not at the same time contain manganese, and when the spar exists in them in considerable quantity, this renders them altogether incapable of being fused, or at least it renders this operation extremely difficult; and in all cases a bad cast metal is obtained, and the iron which is extracted from it by refining, is frequently so brittle when hot as to be altogether unfit for use.

The ores which contain manganese are little or not at all subject to these bad effects of the ponderous spar. Of this I have fully convinced myself at Smalcalde, in the Hanneberg, and at Gittelde, in the Hartz mountains. In the last-mentioned place the ponderous spar is mixed in extraordinary quantity with the spathic ore and the hematites; it is disseminated in them in such minute grains as to be absolutely inseparable from them, and yet these ores are fused with facility, and the iron obtained from them has no bad quality.

M. Quantz has explained this effect of manganese in his treatise on the "Art of working Iron and Steel in the Territory of Smalcalde." He says that the manganese, by means of its oxygene, prevents the entire reduction into sulphur of the sulphuric acid contained in the ponderous spar, and that this acid is volatilized in the form of sulphurous acid.

If manganesiferous ones could be added to the iron ore known in the Hartz by the name of *knollen* (tubercular), it might then be fused and turned to advantage. This Inspector Stuenkel has proved by experiments made on a large scale: it is the ponderous spar, which is mixed with

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it in very large quantity, that renders it incapable of being wrought; were it not for this, it would certainly afford good cast metal. M. Gmelin has analyzed some specimens which were perfectly pure, and has found nothing in them which could produce the quality of being brittle in heat, which the iron extracted from them exhibits; but of a hundred specimens of ore, there are not two which do not contain ponderous spar.

This good effect produced by the manganese in the treatment of the ores mixed with ponderous spar, does not take place equally in that of the minerals which contain martial pyrites; of which the following is an example. Near Altenaner, in the Hartz, there is found an iron ore, which consists of very poor spathic ore and brown hæmatites, the whole mixed with a large proportion of quartz. This ore had hitherto been melted in a neighbouring foundery; but for a year past it has been carried to Lehrbach, where it is mixed with the minerals which are melted in the founderies of that country. These minerals are red, siliceous, and argillaceous iron ore, which contains pyrites. Whatever mixtures have been made of the different varieties of this ore, a cast metal was obtained from it, of which little use could be made, because the iron extracted from it was brittle in heat, or at least because it was necessary to add to it cast iron of a different quality, in order to obtain good iron in bars. This iron was the more brittle in heat as the cast metal was less smooth and less grey, and it never was entirely so. But when manganesiferous ores are added, hardly any more iron, brittle in heat is obtained, however little grey the cast metal may be.

In whatever manner the manganesiferous ores are melted, they always yield that kind of metal which we have termed the *white* cast metal, on account of its co-

lour. I think I have shewn, by a certain consequence, that this cast metal contained manganese. My present object is to specify more particularly the difference between my two species of cast metal.

Of the Cast Metal obtained from Ores which are not manganesiferous, or of the grey Cast Metal.

The colour of the *grey* cast metal is more or less deep, according as the cast is smooth or harsh; that is to say, according as a greater or less quantity of charcoal has been put into the furnace. It appears to be an assemblage of small grains, which are nothing else than crystals. When the cast is smooth, they have a blackish and very brilliant aspect, and are larger than when it is harsh or half-smooth; in the latter case, the fracture is dull, and it has a grey ash-colour.

However, the grey cast metal, in order to present these characters, ought not to be cooled too rapidly; for in this case, which happens when it is cast in thin plates, the smooth cast metal is in small grains, and of a light grey colour; the half smooth is whitish, and the harsh perfectly white.

When the *grey* cast metal is very harsh, it is thick, and flows slowly as it comes from the furnace; it emits a slight sound, and exhibits an ebullition: it quickly becomes solid, and in doing so small vortexes are observed at its surface: this surface, when the cast metal has become solid, is full of small holes or concavities. Its fracture is pretty white, and its internal aspect is neither rough nor granulated, but rather even; the texture is then so close, and the grain so fine, that the crystals can no longer be distinguished. This cast metal is harsh, brittle; it is easily refined, but it undergoes at the same time a very considerable loss, and generally affords an

iron

iron of bad quality. It is a product which it is hardly ever wished to obtain *.

The grey cast metal, when it is very smooth, is likewise thick; but it runs tranquilly as it comes from the furnace, and it becomes fixed with much less rapidity than the preceding: it is as thick as this, but more fusible. Whilst it is running it appears covered with a kind of froth, and it throws up a large quantity of carburet of iron to its surface †; this surface is rough, slightly undulated, but without holes or asperities: the cast metal has a certain degree of ductility, and does not easily break. The refining of it requires a little more time than that of the harsh cast metal; but it undergoes less loss, and yields better iron. Its pastey fluidity renders it full as unfit for being cast into moulds as the harsh metal: that which is best adapted for this kind of work is the

* Great pains are taken to avoid those operations of the furnace which yield this kind of cast metal; not only because the metal is bad, but also because the smelting goes on ill, the scum being often very much charged with iron. Besides, the furnace is then in such a state that a small neglect may have very disastrous consequences; for example, if the blast be a little too strong, or the charge of ore too large, the crucible is then in danger of becoming obstructed; in which case there is no other remedy than to put out the fire.

† I think I have remarked, that the carburet of iron does not begin to appear at the surface of the melted metal until the temperature of the latter has become lower than it was in the furnace. This is easily observed in casting pig-iron; the carburet does not appear until the cast metal is already at a certain distance from the orifice. It appears to me that the carbon is dissolved in the cast metal, and that it comports itself therein nearly like salts in water. When the temperature of this fluid is reduced, a portion of the salt separates itself, carrying with it a quantity of water of crystallization; in like manner, when the cast metal cools, a part of the dissolved carbon separates, and carries with it a small quantity of iron, which constitutes it carburet.

half-smooth species, as it receives best the impressions of the moulds. Frequently also this kind of cast metal is the best adapted for refining, except in cases where the ores contain a matter which tends to render the iron brittle in the cold or heat: in such cases the greyest cast metal is that which affords iron the least subject to these defects, and it is that which we must then endeavour to obtain. The half-smooth species holds the intermediate place between the two others with regard to colour, lustre, and fracture of the grain; it seldom presents any carburet of iron, and then but very little: this latter substance is never found in the harsh cast metal.

The manner in which these three varieties of grey-cast iron comport themselves in the forge is somewhat different with respect to the time of the refining; this difference, however, is inconsiderable; and, by mixing the smooth-cast metal with the brittle, (which is the best and most usual method,) one may very easily, in a German refining fire, where masses of 2¼ quintals are fabricated, refine from 50 to 56 quintals of bar iron in a week, from Monday morning to Saturday noon.

It is totally impossible with the grey-cast metal to make steel good enough to be applied to any useful purpose: of this we have fully convinced ourselves by various experiments, which we have lately made in the Hartz. The reason is, that when this cast metal is wrought for the purpose of converting it into steel, it passes too easily into the state of refined iron.

TO BE CONCLUDED IN OUR NEXT.

Observations

Observations on the Combination of Antimony with Tin.

By M. THENARD.

From the ANNALES DE CHIMIE.

ABOUT a year ago I had occasion to make an observation, of which I am about to give an account, in examining an alloy which was attempted to be introduced into commerce, and to which such valuable properties were erroneously attributed, as might, if it had really possessed them, have rendered it in many respects a fit substitute for silver.

It was said to be very malleable; but what was chiefly extolled was its unalterability. From its appearance, I conjectured that it must have contained a large proportion of tin, and as its price was moderate, I also suspected the presence of antimony in it, and to a certain degree that of zinc or of lead. Under this idea I treated 100 parts of it with nitric acid. They were soon attacked with a violent effervescence, and converted into a white powder. At the end of half an hour's ebullition, after having filtrated the liquor, I tried it successively with potash of commerce, sulphuric acid, and the hydrosulphurets.

All these re-agents having indicated that it contained nothing metallic in solution, I considered it very probable that this alloy was composed merely of tin and antimony. To ascertain this point, I took the above-mentioned white powder, into which it had been converted by the nitric acid, and dissolved it in muriatic acid. I concentrated the solution, and diluted it with water. There was produced, as I had foreseen, a very abundant precipitate; but having let the liquor stand for a day, and

and then decanted it, I hardly found any more traces of metal in it than in the preceding. Ammoniac scarcely rendered it turbid, and the hydro-sulphuret of potash gave it a slight yellow tinge. Though I had performed the operation with great care, I could not give credit to this result, so singular did it appear to me; and it was only by repeating it that I was convinced that I had not been deceived. What could I conclude from this? From considering merely the known properties of the oxyd of antimony and of tin, I must have concluded that the substance which I examined was nothing else than antimony. Nevertheless it was sensibly malleable. It must therefore contain some other metal. Every thing seemed to pursuade me that this was tin; and, in fact, I formed with antimony and tin an alloy which possessed entirely the same properties. Four parts of tin and one of antimony gave a very ductile one. From equal parts there results an alloy which still possesses a certain degree of ductility. But if a few hundredth parts of lead are added to the tin, both become very brittle. The intermediate alloys enjoy properties relative to the quantities of tin and of antimony which they contain. None of them are precipitated from their solution in nitro-muriatic acid by water. There necessarily exist certain limits, but these limits are very remote. It is sufficient that the alloy contain one-third of its weight of antimony to be in this predicament, especially if the excess of acid has been in a great measure expelled by evaporation. I must, however, mention that the precipitation is not quite completed till at the end of twenty-four hours, when the antimony predominates; for then the last portions, which are a combination of the two oxyds with the muriatic acid, separate themselves but gradually.

This,

This, however, is not the only instance which we have of combinations of oxyds. It cannot be doubted, that the oxyd of tin combines itself with the oxyd of lead; for in calcining an alloy of three or four parts of lead with one part of tin, it soon burns after the manner of a pyrophorus, and is suddenly converted into oxyd, whereas tin, which is much more combustible than lead, far from presenting this phenomenon, is not converted into oxyd without requiring a considerable space of time, even when its points of contact with the air are multiplied. I have made many other experiments to ascertain whether the oxyd of antimony acted upon other oxyds like upon the oxyd of tin; and I think I have convinced myself that this is the only one which the antimonial oxyd carries into precipitation, and that the oxyd of bismuth precipitates none with it, not even the oxyd of tin.

After having thus proved by synthesis that my alloy was formed of tin and antimony, it was necessary that I should seek for the analytical means proper for effecting the separation of these two constituent principles. I first employed the muriatic acid which dissolves tin well, and does not attack the oxyd of antimony. Its action upon the alloy amounted almost to nothing; moreover, in the portion dissolved I again found antimony. I then tried sulphurated hydrogene. I knew that it easily precipitated the muriate of antimony, and that, on the other hand, it decomposed highly oxydated muriate of tin with difficulty. This means did not prove more successful than the first. The liquid concreted into a mass, and did not permit me to obtain the salt of tin by itself, and even the separation was far from being exact. I also tried, though without success, to volatilize the antimony, by

by strongly heating the alloy in close vessels. Finally, I treated the alloy by the nitro-muriatic acid, and I took care that the two muriates were highly oxydated. I distilled them in a retort, taking care to stir them continually, especially towards the end of the operation. I even urged the fire till the bottom of the retort became red hot, and I sensibly obtained the whole of the muriate of tin. The muriate of antimony which, when highly oxydated, is not volatile, remained in the retort. Only a very small quantity of this salt had passed into the receiver. Also the muriate of tin was scarcely rendered turbid by water. Though this process is perhaps not a very accurate one for separating the two oxyds of tin and of antimony, I consider it as the best that can be employed, and conceive it to be sufficiently exact to indicate some hundredth parts of tin which the antimony might contain, or some hundredths of antimony that might exist in the tin.

The inferences to be drawn from this new fact, which is of sufficient importance to merit the attention of chemists, are evident. Is it very certain that the antimonial ores do not contain tin, and *vice versâ*? Assuredly not, since the modes of analysis hitherto employed for separating these two metals are inaccurate. I have already sought for tin in the sulphuret of antimony, and have found no trace of it; but I have not yet analyzed the other ores of antimony any more than those of tin. This is an inquiry which I should think it very useful to make, and which might perhaps lead to interesting results.

New Observations on the Nature of Tannin.

By M. TROMSDORFF.

(Concluded from Page 67.)

Experiment XXV.

THE precipitate obtained (Experiment XXIV.) by the nitrate of lead was diluted with distilled water, and then exposed to a continued stream of acid of hydrothion. The liquor assumed a dark-brown colour, and yielded a pure tannin, after having been filtered and evaporated to dryness. The residue being treated a second time in the same manner with acid of hydrothion, again left pure tannin; but the third time the water scarcely assumed a yellowish tinge. The tannin obtained in this manner was very soluble, and exhibited not the slightest attraction for the moisture of the atmosphere, after having been dried by evaporation.

Experiment XXVI.

I then occupied myself with the very sour liquor, which had been decanted from the precipitate of Experiment XVIII, and I added to it the water of lixiviation. On mixing solution of glue with it, I obtained an abundant precipitate, which proves that it still contained a large quantity of tannin. In order to obtain the latter, I first evaporated the water from it; but the fire not having been slackened in time, sulphureous acid was formed, and only a coaly substance was obtained, which retained none of the properties of tannin.

These experiments, well considered, lead to the following inferences. 1. It is true, as Proust says, that the sulphuric acid produces a precipitate in a concentrated

solution of galls. 2. It is not yet demonstrated, that this precipitate is a combination of sulphuric acid and tannin; it may, on the contrary, be only tannin in an altered state which has retained a small quantity of sulphuric acid; what renders this probable is, its but slightly acidulous taste, and the small quantity of potash necessary for its saturation. The addition of potash not only carries off the combined acid, but it also in part restores the tannin to its original state, for I really obtained (Experiment XXI.) a pure tannin by means of alcohol. The addition of potash would therefore not be necessary (as Proust thinks) to decompose the sulphate of tannin, but rather to restore the tannin to its original state; and an excess of potash would even be detrimental, since it would enter into combination with the tannin, as is proved by Experiment XXII. Moreover, Experiment XXIII. proves that a part of the tannin is so altered in its nature by the effect of the acid, that it even loses its action upon the solution of glue. In order to attain to greater certainty in this obscure matter, it would be necessary to determine well, whether the precipitate obtained by the sulphuric acid is really a combination of the sulphuric acid with the tannin, or whether this acid adheres to it only mechanically, and whether the tannin is separated in consequence of the sulphuric acid oxydating it, or subtracting hydrogene or carbon from it.

Experiment XXVII.

I prepared a new watery infusion of galls, which I concentrated by evaporation to such a degree that it became turbid in cooling, and a small quantity of tannin was precipitated. I gradually poured into it pure sulphuric acid, whence resulted precisely the phenomenon of Exp. XVIII. When the precipitate had deposited itself I poured off the

the supernatant liquor, which was still yellow, and of a very sour taste. Upon the residue I poured a small quantity of distilled water, in which I stirred it well, and then poured off the water. I used the precaution to add little water at once, and to take it very cold, at twelve different times. It is true this caused a loss of much precipitate, but then not the slightest acid taste was left. This precipitate having been dried, there resulted from it a brown mass, which no longer attracted the humidity of the air. Cold water dissolved but a very small quantity of it, and it required at least seven times as much hot water as there was pure tannin, to effect its solution; yet there still remained something that was not dissolved, but this hardly amounted to more than 0,09 of the whole. This solution precipitated that of glue like pure tannin; however, the precipitate was less tenacious than that of pure tannin, and of a deeper colour. Its action upon the solutions of iron and other metals was the same as that of tannin. The muriate and the acetate of barytes did not render this solution in any degree turbid; whence I think we may certainly conclude the absence of the sulphuric acid. Water of barytes, it is true, was precipitated; but as the precipitate dissolved intirely in nitric acid, it is evident that no sulphate of barytes had been formed. I repeated the evaporation to dryness with the rest of the solution, the residuum of which I covered with rectified alcohol, without any solution taking place; on adding water to it, I observed that the solution was now effected with much greater facility. When I had evaporated the alcohol and water to dryness, the brown mass which resulted dissolved much more easily in distilled water, but yet not quite so easily as pure tannin does.

These experiments appeared to me to prove that the sulphuric acid is not precipitated in combination with the

tannin, but that there merely results from it a change (the nature of which, indeed, I cannot yet determine) in the composition of the tannin, by means of which the latter dissolves with greater difficulty in water, but retains its other essential properties. Hence it also results, that alcohol restores its solubility to a certain degree, which may serve to explain the nineteenth experiment. The alkali is therefore by no means necessary when the adhering acid has been separated; on the contrary, its addition determines a combination with the tannin, which renders it insoluble in water, (Experiment XXII.) To ascertain the disadvantage arising from the alkali I made the following experiment.

Experiment XXVIII.

I took the tannin obtained in the preceding experiment, and, after having dissolved it in water, I added a little carbonate of potash, which immediately rendered it turbid. I evaporated to dryness, and put the residuum to digest with alcohol containing $\frac{2}{100}$ of water. This liquid took up but very little of it; and what remained upon the filtre comported itself exactly like the mass of Experiment XXII. The spirituous liquid, evaporated, left a scarcely perceptible quantity of tannin.

Experiment XXIX.

The acid liquor (Experiment XVII.) and the water which had served to wash the residuum were carefully evaporated to a fourth, and then exposed to cold, whence resulted an abundant dark-coloured precipitate, of which a part was lost in repeating the cold washing, but what remained was altogether of the same nature as the precipitate of the first washing, (Experiment XXVII.)

Experiment

Experiment XXX.

As Proust also makes mention of the muriatic acid, as a means of precipitating tannin, with which it is likewise to combine after the manner of the sulphuric acid in its precipitation, I resolved to make an experiment on this subject. I poured into a highly saturated infusion of galls, pure and concentrated muriatic acid, till all precipitation ceased. The precipitate perfectly resembled that which is produced by the sulphuric acid. As soon as it was well collected at the bottom, I decanted the liquid, which I put by, and marked it No. 1; as to the precipitate, I washed it ten different times with distilled water, using the precaution, as in the preceding experiment, to employ each time only very cold water, and in very small quantity; notwithstanding which a third of the precipitate disappeared. It now had no longer any acid taste, it dissolved in boiling water, but employed in such large quantity that the solution was not more coloured than a weak infusion of galls; its taste was purely astringent, it precipitated animal jelly abundantly, it threw down the muriate of iron in a black precipitate; but it produced no turbidity in the solution of muriate of silver, and consequently gave no longer any indication of muriatic acid. To obviate the objection, that perhaps the muriatic acid intimately combined with the tannin had no more action upon the muriate of silver, I put into the solution of tannin some drops of pure carbonate of potash, completely deprived of muriatic acid; whence resulted indeed a precipitate, but this was not muriate of silver, for it dissolved with ease in nitric acid. I then evaporated the other solution to dryness, I lixiviated the product with alcohol diluted with a tenth of water, I heated them together, letting the alcohol evaporate; the residuum dissolved very well in water, and presented all the other properties of tannin.

The

The preceding experiments convinced me that the data of Proust relative to these phenomena were inaccurate; moreover, they taught me that, by means of the acids, at least of the sulphuric acid, the separation of the tannin may be effected, although this method is by no means to be recommended; 1, because, supposing the infusion of galls to be as much concentrated as possible, there is always a large portion which, remaining dissolved in the water, is lost; 2, because much of the tannin precipitated is lost also in the repeated washings.

Experiment XXXI.

I next thought it necessary to examine the state of my pure tannin (Experiment XVI.) in relation to the sulphuric and muriatic acids; for it was very possible that its result might be different, since, besides the gallic acid, the infusion of galls contains mucilage, extractive matter, and calcareous sulphate. I therefore dissolved one part of my pure tannin in three parts of water, and on treating it with the sulphuric acid I remarked exactly the same phenomena as in the saturated infusion of galls; I obtained only a fourth of the tannin employed, the surplus having remained in the acid liquor, and in the water of lixiviation. With the muriatic acid, the result were the same as in the infusion of galls; it would be superfluous to repeat the processes in this place.

It now remained for me to ascertain how the acids rendered the tannin in part so difficult to be dissolved. I thought I could not attempt the solution of the question with better prospect of success than by investigating the effect of several acids in a different state of oxydation upon the infusion of galls. With this view I undertook the following experiments, which however, as will soon appear, did not yet lead me to a satisfactory solution.

Experiment

Experiment XXXII.

I conducted, during a certain space of time, a stream of sulphurous acid gas into a saturated infusion of galls; but I observed no other change than the absorption. Neither the colour nor the transparency of the liquid was altered, nor was any precipitate thrown down. The solution of glue was precipitated by means of it as well as before, and the coriaceous combination had the known properties. Thus the sulphurous acid manifested no action upon tannin.

Experiment XXXIII.

Eight ounces of a highly-concentrated solution of galls were mixed with an ounce of crystallizable acetic acid, (prepared according to the process of Lowitz): neither precipitate nor any other change was produced. An ounce of pure tannin dissolved in four ounces of distilled water, and mixed with concentrated acetic acid, gave the same result. The acetic acid had therefore no more action upon the tannin, since the infusion acted as before upon the solution of glue.

Experiment XXXIV.

Pure concentrated phosphoric acid (composed of about one part of acid to one and a half of water) likewise produced no alteration in an infusion of galls.

Experiment XXXV.

Crystallized oxalic acid, heated with six times its weight of concentrated tincture of galls, produced no alteration. The case was the same with the tartarous and malic acids, both of which changed neither the highly-saturated infusion of galls, concentrated by evaporation, nor the solution of pure tannin.

Experiment

Experiment XXXVI.

Nitric acid produced no precipitate in the infusion of galls. I added to it, drop by drop, fuming nitric acid, which produced a great heat; the liquid assumed a superb red colour, was brought to ebullition by the addition of fresh acid, became yellow, and at last straw-coloured, yielding an acidulous liquor similar to malic acid.

Experiment XXXVII.

I caused a stream of muriatic acid gas to pass into a saturated tincture of galls; after some time a precipitate was formed similar to that of Experiment XXX, which accords also with the other observations.

Experiment XXXVIII.

I poured liquid arsenic acid into a saturated infusion of galls. It produced an abundant precipitate, which was first washed several times with cold water, whereby more than one half was made to disappear. Having afterwards poured boiling water upon it, a complete solution was formed, which precipitated that of glue in the ordinary manner, and had the quality of tannin.

Experiment XXXIX.

I caused oxygenated muriatic acid gas to pass into a saturated infusion of galls. After some time the infusion assumed a deeper colour, but without becoming turbid, and it still precipitated, without any change, the solution of glue; however the precipitate was of a gold-yellow colour. I again caused a considerable quantity of oxygenated muriatic acid gas to pass into this infusion, which had acquired a deeper colour, and the result was a brown precipitate, very difficult to be dissolved in water, and which did not precipitate the solution of glue; whilst

whilst the supernatant liquor still acted as tan-water upon the same solution.

I have already observed, and proved by experiment, that the alkalies do not precipitate the pure tannin from its solution, because they attract the water, as Proust has imagined. The following experiments will afford farther proof that the tannin enters into a true chemical combination with the alkalies and the earths.

Experiment XL.

One part of pure tannin was dissolved in three parts of distilled water, to which was added fresh-prepared caustic alkaline ley, till no more precipitate was thrown down. I decanted, and dried the precipitate. It was soluble in a large quantity of boiling water, but it did not precipitate the solution of glue except when the alkali had been saturated with an acid. The case was the same with the decanted liquid. When carbonate of potash was used instead of the caustic alkali, the result was the same. Potash forms therefore with tannin a compound which is more difficult of solution in water than pure tannin, and which, for this reason, is in part precipitated. The result is the same with soda.

Experiment XLI.

With a concentrated solution of pure tannin in distilled water, caustic ammoniac was mixed till the presence of the latter discovered itself by its pungent odour; no precipitate indeed was thrown down, but neither did this mixture precipitate the solution of glue. No sooner had I added an acid to it than a precipitate was immediately formed. I then evaporated the liquid to dryness, which volatilized a large quantity of ammoniac, and gave as residuum a shining black powder, which was scarcely astringent to the taste.

This powder dissolved readily in hot water, but its solution did not precipitate that of glue till after the addition of an acid. In triturating this powder with caustic potash or carbonate of potash, the odour of ammoniac became very distinctly perceptible, and it consequently remains in real chemical combination with the tannin.

Experiments XLII. and XLIII.

Fresh lime-water produces a considerable precipitate in a solution of pure tannin, and the liquor becomes quite colourless. The precipitate is a compound of tannin and lime, which dissolves with difficulty in water, and does not precipitate the solution of glue unless when the lime has been taken up by an acid. The case is the same with the water of barytes.

Experiment XLIV.

When a fresh precipitate of alumine is agitated with a solution of pure tannin, a part of the tannin unites with the earth, and can no more be separated from it by boiling water. If we dissolve in dilute sulphuric acid this combination, which forms a powder of a greenish-grey, the tannin is again set free, and acts upon the solution of glue.

Experiment XLV.

Magnesia acts upon the solution of tannin in the same manner as alumine; however, it takes up less of it. The tannate of magnesia is of a dirty yellowish-brown colour; it is insoluble in water, and the acids decompose it with ease.

I propose to resume this inquiry as soon as my other avocations shall permit; principally with a view to ascertain the manner in which tannin comports itself with the metallic substances.

Report

Report on a new Process for the Preparation of the Muriate of Barytes, and on the Means of depriving the Brandies of Potatoes, of Hops, and others, of their disagreeable Smell. By M. RESAT.

Read before the Society of Pharmacy of Paris, by Messrs. BOULLAY *and* PLANCHE.

From the ANNALES DE CHIMIE.

SOME time ago M. Parmentier communicated to the Society a letter which he had received from M. Resat, practical chemist of Remiremont. This letter contains among other observations, 1, a new process for the preparation of the muriate of barytes; 2, a method for depriving the brandies of potatoes, of hops, and others, of their disagreeable odour, and by the addition of a small quantity of sulphuric acid and a new distillation.

The Society, who appeared to take an interest in these two observations, having solicited M. Boullay and me to repeat the experiments of the author, and report to them the result, I have taken upon myself to fulfil this latter part of our commission, and trust that what I have to offer will be considered by the Society with indulgence.

I. *On the Muriate of Barytes.*

M. Resat, after having tried different processes indicated for preparing the muriate of barytes, among others those of M. Hufeland and of our colleague M. Bouillon Lagrange, not being satisfied with the quantities of salt obtained by either of these means, or with the considerable consumption of combustible, and the loss of time, engaged in researches for the purpose of remedying these inconveniencies, the following is the process which he found to succeed the best.

Take sulphate of barytes, pulverized, four parts; charcoal, also pulverized, one part; flours of sulphur a third of a part. Less time by half, the author asserts, is required, than in the known processes, and one-twelfth more of crystals is obtained from the first crystallization. As M. Resat has not entered into any details respecting the mode of operating, the quantity of salt obtained from the processes already known to the public, and from that which he proposes, finally respecting the time his operation must last, we thought it necessary, in order to arrive at a pretty correct judgment upon this subject, to repeat, for the sake of comparison, the process, which has for a long time past been practised in the laboratories, that of M. Bouillon Lagrange, and that of the author, of which we shall now give accounts in succession.

Old Process. We kept at a red heat, for three hours and a half, in a reverberatory furnace, a Hessian crucible filled with a mixture composed of sixteen ounces of sulphate of barytes, two ounces six drachms of charcoal, both pulverized, and wetted with a sufficient quantity of essence of turpentine, (about two ounces).

When the crucible had half-cooled, the sulphuret of barytes was mixed with twelve pounds of pure hot water; we then filtrated and precipitated by the muriatic acid.

The liquor separated from the precipitate by a new filtration was evaporated till a slight pellicle was formed.

	oz.	drs.
It gave by the first crystallization muriate of barytes, pretty pure - - - - - - - - -	2	5
Second crystallization, muriate ditto less white	2	2½
Third crystallization, muriate similar to the preceding - - - - - - - - - - - - -	0	4

The mother-water having been evaporated to a great degree, yielded a shapeless mass, of a yel-

	oz.	drs.
a yellowish colour, composed of muriate of barytes and of iron, weighing - - - - -	0	1½
In all - - - - - -	5	5

The product of these three crystallizations, purified by the known means, yielded crystallized muriate of barytes of great purity, 4 oz. 7 drs.

Process of M. Bouillon Lagrange. We mixed equal parts of sulphate of barytes and purified muriate of lime, (16 ounces of each.) This mixture was projected by spoonfuls into a crucible heated to redness. It ran twenty minutes before the matter was in complete fusion.

We followed, in the rest of the operation, the precautions recommended by the author. The products which we obtained were,

	oz.	dr.	gr.
First crystallization, muriate of barytes very white, in pretty regular crystals, containing a very small quantity of muriate of lime,	3	4	24
Second crystallization; the same salt in small laminæ, resembling boracic acid, as white as the preceding - - - - - - - - -	1	4	36
The decanted liquor, evaporated anew, yielded 3 dr. 40 gr. of barytic salt, irregularly crystallized, very deliquescent, and in which the oxalic acid discovered to us the presence of 26 grs. of muriate of lime - - - -	0	3	40
Total weight of the crystals - -	5	4	28

The mother-water, evaporated to dryness, was discovered to be muriate of lime mixed with an almost inappreciable quantity of muriate of barytes.

We

We shall remark, on this occasion, that the presence of a very small portion of this salt seems singularly to favour the crystallization of the calcareous muriate.

The salt obtained from the preceding crystallizations, produced, after being again dissolved and evaporated, crystallized muriate of barytes of great purity 5 oz. 1 dr. 40 gr.

Process of M. Resat. We mixed accurately 16 ounces of sulphate of barytes, four ounces of powdered charcoal, one ounce two drachms and forty-eight grains of flowers of sulphur, being the proportions indicated by the author. This mixture was heated for the space of half an hour in a crucible closed and luted. The matter was then mixed with twelve times its weight of pure hot water; the solution, filtrated, was precipitated by the muriatic acid added in excess; the liquor was again filtrated and heated, in order to disengage from it the whole of the sulphurated hydrogene, at the same time with a considerable quantity of hydrogenated sulphur, which had been precipitated, and which we separated by the filtre.

The evaporation, carried as far as the formation of a slight pellicle, produced:

	oz.	dr.	gr.
First crystallization: muriate of barytes very pure, such as we now present to the Society	3	2	32
Second crystallization: muriate a little less white than the preceding	1	4	48
Lastly: crystals of a dirty white tinge	1	1	60
In all	6	0	68

The

The product of the second and third crystallizations purified by a new solution was 2 oz. 4 drs. 12 grs. which united with the 3 oz. 2 drs. 32 grs. obtained from the first, give for the quantity of pure muriate of barytes obtained from one pound of sulphate, 5 oz. 6 drs. 44 grs.

On examining the washed matter that had remained upon the filtre after the solution of the sulphuret of barytes, we found that there was still left a considerable quantity of sulphate not decomposed. This observation led us to suspect that the matter had not been sufficiently heated, although the author of the new process announces that less time by half is required than according to the known methods. This assertion which, at first sight has some appearance of probability, if compared with the old processes, is entirely destitute of foundation when we oppose to it that of M. Bouillon Lagrange, for which from 15 to 20 minutes are sufficient, especially if we operate upon no very considerable mass.

These considerations induced us to recommence the operation in the same proportions as above; and in order that all the circumstances might be rendered as similar as possible, we heated the crucible in the same furnace which had served for the old process, during the same space of time and with the same quantity of fuel, within a very trifling difference.

The result of these experiments shewed us that a larger quantity of sulphate had indeed been decomposed, since instead of 5 oz. 6 dr. and 44 grs. of pure muriate, which the preceding operation yielded, we obtained in this 7 ounces and 12 grains of the same salt.

If we now compare the quantities of muriate of barytes furnished by the three processes, for each of which 16 ounces of sulphate of barytes were employed, we find them in the following relations:

1, Old

	oz.	dr.	grs.
1, Old process -	4	6	0
2, Process of M. Bouillon Lagrange	5	1	40
3, Process of M. Resat -	7	0	12

We are very far from pretending to present these results as strictly invariable; for every one knows, and chemists particularly, that operations of this kind are liable to be influenced by a multitude of circumstances. Such are the application of the caloric, always unequal, whatever precautions may be used, the state of vacuity of the crucible, the more or less considerable interval existing between each particle of the heated matter, &c.

We think, however, that this sketch will serve to direct the pharmaceutical operator in the choice of the process which he may consider the most advantageous; and upon this supposition we shall offer the following reflexions:

1, The process of M. Resat has over the old one the advantage of furnishing a larger quantity of muriate of barytes than can be obtained pure after the first crystallization; 2, If we shall be enabled to burn the large quantity of sulphurated hydrogene gas which is disengaged during the operation, there is no doubt that the new process will obtain the exclusive preference.

These reflections will more particularly interest practitioners of pharmacy who, embarrassed by local circumstances, cannot engage in operations of this kind without running some hazard for themselves, and exposing themselves to the reproaches of their neighbours who are incommoded by these fetid exhalations. Such will without hesitation give the preference to the process of M. Bouillon Lagrange, a preference founded upon the security of the operator, the celerity of the operation, the actual saving

saving of fuel, and finally upon the means of applying to a useful purpose a salt of little value, and which almost always abounds in the laboratories.

II. *On the Means of depriving Brandies of their disagreeable Smell, especially those prepared from Potatoes and from Hops.*

"At the time when brandies were very dear, says M. "Resat, I have tried different means for removing the "odour of those prepared from potatoes and from hops. "I shall here describe that which I have found the most "successful. Into 50 kilogrammes of brandy of a bad "flavour, I poured 5 hectogrammes of sulphuric acid. "After having stirred the mixture, I let it stand twenty-"four hours and then distilled it. The spirit obtained "was without any perceptible smell of ether; the acid "had oxydated the oil to such a degree as to render it "fixed."

Such is the description of the process employed by M. Resart.

In order to repeat it, we endeavoured, but in vain, in various distilleries to procure brandies prepared from potatoes and from hops. In want of these, and under the persuasion that the process of the author being applicable to brandies of bad flavour, that known in commerce by the name of *eau-de-vie de marcs*, would equally afford us means of ascertaining its value, we contented ourselves with the latter.

For this purpose we put into the water-bath of a small alembic five pounds of bad brandy (eau-de-vie de marcs) of a disagreeable smell and taste, marking 24 degrees of Baume's areometer. We added and mixed with it a little more than $\frac{1}{100}$ part of concentrated sulphuric acid,

and after letting it stand for twenty-four hours, we proceeded to the distillation, whereby we obtained 48 ounces of alcohol, which then marked 36 degrees of the areometer.

This liquor we found to possess the following properties: 1, Its smell and taste were much less disagreeable than previous to the distillation. 2, Mixed with a third of its volume of pure water, the original taste re-appeared with almost equal intensity, though its bad taste was sensibly diminished.

We think however that, notwithstanding the process of M. Resat, by means of which the brandies are deprived of a part of their empyreumatic odour, as is proved by the analogous one which the residue exhales; persons accustomed to try brandies by tasting, will not easily be deceived respecting the real origin of the alcohol rectified by the process which the author proposes. Though we cannot admit with him that oily matter contained in the alcohol becomes fixed in consequence of its oxydation, which is more naturally explained by the precipitation of the carbon with the aid of the sulphuric acid; we believe that the process of the author, imperfect as it is at present, deserves to be better known to chemists. We therefore conclude: 1, That the observations of M. Resat, the first upon the muriate of barytes, and the second upon the brandies prepared from potatoes and from hops, ought to be published in print; 2, That the Corresponding Secretary of the Society should be directed to transmit the thanks of the Society to the author, and to solicit his future communications.

Analysis

Analysis of James's Powder. By M. PULLY.

Communicated by M. CADET.

From the ANNALES DE CHIMIE.

JAMES's Powder is in high vogue in England and Italy: the English make a secret of its preparation, and sell it at as high a rate as its weight in gold, as a sovereign remedy in asthenic diseases, and fevers attended with a depression of the vital energy. Dr. Pearson, an English chemist, has, as he says, analyzed it, and he has asserted in print that this powder is a triple salt, composed of phosphate of lime and oxyd of antimony. The chemists who have attempted to compose James's Powder according to Dr. Pearson's analysis, have undoubtedly been surprised that they did not succeed; but they might naturally suppose that the English physician, unwilling to betray a secret lucrative to his country, used some concealment in the matter; in fact, James's powder contains, besides, sulphate of potash and antimoniated potash *.

M. Pully, a Neapolitan chemist, who, in his capacity of administrator of the gunpowder and saltpetre works, has rendered great services to the army of Italy, writes me, that he has been making the analysis of this powder, of which he had procured a sufficient quantity from London to subject it to a rigorous examination. The following is a sketch of his experiments.

* Dr. Pearson's object in his analysis was certainly not the concealment, but the publication of the composition of James's Powder. The only persons interested in keeping this a secret are the proprietors, with whom the doctor, we believe, has no connection. Although a patent was originally obtained for this powder, and a specification duly enrolled, it is well known that the medicine cannot be prepared from the description there given of its composition, which, like those we find in most specifications of medical patents, is, contrary to the original intent and express condition for which patents are granted, purposely rendered unintelligible, with a view to prevent imitation.

Experiment I.

I took (he writes) nineteen decigrammes of James's Powder, and put them to infuse for some minutes in a small quantity of hot distilled water. I separated the water from them by the filtre, and I obtained, by the evaporation of this liquid a salt, which had all the characters of the sulphate of potash. This salt, dissolved and treated with barytes, presented a precipitate of sulphate of barytes.

Experiment II.

As I had perceived, before decomposing the salt by barytes, that the solution contained an excess of free potash, I wished to ascertain whether this potash did not hold in suspension a small quantity of oxyd of antimony. Having decanted the liquor in order to separate it from the sulphate of barytes, I poured into it sulphurated hydrogene, which immediately formed golden sulphur of antimony; consequently the free potash was combined with a portion of antimony at the *minimum* of oxydation. Dr. Pearson speaks neither of this combination nor of the sulphate of potash.

Experiment III.

I took the James's Powder which had been washed with hot distilled water, and heated it with nitric acid of 20 degrees. This acid dissolved the phosphate of lime without attacking the oxyd of antimony at the maximum. I separated this oxyd from the solution, and poured into the liquor ammoniac, which precipitated the phosphate of lime.

Experiment IV.

I decomposed the phosphate of lime by weak sulphuric acid, and afterwards re-composed it by lime-water, in order to determine its proportions.

Experiment

Experiment V.

I took the oxyd of antimony at the *maximum* of oxydation, and dissolved it in muriatic acid. This solution, treated by sulphurated hydrogene, produced a hydrosulphuret of antimony, containing more sulphur than the Kermes mineral, and less than the sulphur auratum.

According to these experiments, of which I weighed all the products, the nineteen decigrammes of James's powder which I analyzed were composed of

	Decigrammes.
Oxyd of antimony, at the maximum - - - - -	7
Phosphate of lime - - - - - - - - - - -	4
Sulphate of potash - - - - - - - - - - -	4½
Free potash, containing oxyd of antimony at the minimum - - - - - - - - - - - - -	3½
	19

To re-compose this powder, we must take,

Sulphate of antimony - - - -	2 parts
Calcined phosphate of lime - -	1½
Nitrate of potash - - - - - -	4

These substances are to be pulverized, mixed, and triturated. They are then to be put into a crucible, which is to be closed, and strongly heated. During this operation, the oxygene of the nitric acid, acting upon the sulphur of the sulphuret of antimony, converts it into sulphuric acid, which unites with a portion of the potash, and forms sulphate of potash; the rest of the free potash retains antimony oxydated at the minimum. The white powder which remains in the crucible is the same as that which is sold at such a high price by the English.

M. Pully asserts that he has analyzed his powder, in order to compare it with that of James, as it is called, and that he has found it to contain the same principles and the same proportions.

Decomposition

Decomposition of the Muriatic Acid.

By M. F. R. PACCHIANI, *Professor of Natural Philosophy at the University of Pisa.*

From the ANNALES DE CHIMIE.

THERE has just appeared at Pisa, a letter from Dr. Pacchiani to Sig. Pignotti, dated May 9, 1805, in which he gives a rapid sketch of the processes which have led him to this important discovery. We shall give an extract of it sufficiently ample to satisfy the just impatience of our readers *.

By occasion of the premium proposed by *the great general and philosopher Bonaparte, Emperor of the French,* for the advancement of that new and prolific branch of natural philosophy which was created by the celebrated Galvani, of Bologna, and since extended by the genius of Volta, I contrived a great number of experiments for accomplishing this purpose; and they have made me acquainted with a number of facts, which I intend to collect in a memoir, and lay before the Italian Society. The following are those which have led me to the discovery of the constituent principles of an acid which has hitherto eluded the researches of chemists. I mean the muriatic acid, which has been in vain tormented with fire, the electric spark, and the affinities. We know how different opinions have been upon this subject; some supposing it to be a simple combustible substance; others, an unknown base, united with oxygene; others again, a principle naturally acid. But these opinions have been looked upon by the adepts in the art as mere hypotheses destitute of proof.

* Lettera del doctor Francesco Pacchiani, professor di fisica nell' Universita di Pisa, etc.

I conceived

I conceived that the continued action of the pile of Volta might afford a new means of attempting this decomposition; nor did my hopes deceive me, for I have arrived at the discovery of the constituent principles of this acid.

The apparatus, and the processes which I have used, are so simple, that they do not leave room for the slightest suspicion of illusion. I shall soon publish a detailed account of the numerous experiments which these researches have required; for the present I shall confine myself to the facts, which evidently establish the following propositions.

1. *The muriatic acid is an oxyd of hydrogene*, consequently composed of hydrogene and oxygene.

2. *In the oxygenated muriatic acid, and therefore, a fortiori, in muriatic acid, there is a much less proportion of oxygene than in water.*

3. *Hydrogene is susceptible of a great many different degrees of oxydation*, not merely of one degree, which forms water, as most of the pneumatic chemists have imagined.

Having observed the phenomena of the decomposition of water by the electric pile, and discovered its true theory by accurate experiments, I easily contrived a simple and convenient apparatus, with which I could easily observe the changes which the water experienced, whilst the continued action of the electric pile separated the oxygene from it, at the surface of a wire of very pure gold which was immersed in it.

In examining the progress of these changes, I observed at last a very singular one, which announced, in an unequivocal manner, the formation of an acid. Other experiments had previously shewn me that the air obtained was very pure vital air, giving scarcely a residuum of $\frac{1}{60}$ in M. Giobert's eudiometer.

I next

I next endeavoured to ascertain, in a more positive manner, the existence and the nature of the acid, of which this decomposition had given me indications.

When the water, or, to speak more accurately, the remaining liquor, occupied not more than about half the capacity of the receiver which contained at first the pure water, it presented the following characters.

Its colour was of an orange-yellow, more or less deep, according as the volume had been more or less reduced; it resembled a real solution of gold.

At the lower orifice of the vessel, which was closed with a piece of taffety, covered with a double bladder, a smell was perceived, which immediately called to mind that of the oxygenated muriatic acid.

The gold wire had in part lost its metallic lustre, and its surface resembled that of metal attacked by a solvent.

A piece of woollen cloth, placed in contact with the coloured liquor, was easily reduced into small fragments, as happens with a substance half-burnt.

Round the edges of the vessel there was formed upon the bladder a ring of a deep purple colour, which bounded a circular space, rendered entirely colourless or white.

A drop of this liquor, applied to the skin of the hand, left, after some hours, a beautiful red tinge.

The liquor obtained by this process, having constantly presented the same characters, I reserved the product of the last operation, in order to subject it to chemical experiments. In these M. Joseph Branchi, a very able chemist of our university, had the goodness to lend me his assistance; they were made in his laboratory, where we easily obtained proofs:

1. That there existed in the substance a volatile acid, which gave white vapours when ammoniac was approached to it.

2. That

2. That this acid was oxygenated muriatic acid, since it precipitated the nitrate of silver in a cheese-like form, and produced the *luna cornea* of the ancients, or muriate of silver.

Hence it results, beyond contradiction,

1. That the muriatic acid is an oxyd of hydrogene, and consequently composed of hydrogene and oxygene.

2. That hydrogene is susceptible of several degrees of oxydation, one of which constitutes water; the next, oxygenated muriatic acid; in default of the latter, that which produces muriatic acid.

I shall treat of the other degrees in a memoir which I intend soon to give to the public.

Experimental philosophy has long been prolific in wonders: the changes of azote into nitric acid, of hydrogene into water, seem to me to approach to the miraculous. I leave it to any one to judge, whether we ought not to place in the same rank that of water into a solvent of gold and of platina, into that volatile substance which attacks and neutralizes the pestilential miasmata, and which affords so many resources to natural philosophy and the arts.

After having discovered the elementary principles of this so refractory substance, I am about to occupy myself with ascertaining their proportions by experiment and calculation.

The nature of the muriatic acid being known, there can remain no more obscurity respecting its origin, or that of the muriatic salts which exist in the vast extent of the ocean. I intend, upon a future occasion, to deduce from these data a variety of other inferences.

L. B. G.

Process for obtaining Cobalt pure. By M. TROMSDORFF.

From GEHLEN'S CHEMICAL JOURNAL.

MIX carefully four parts of well-pulverized zaffre with one part of nitrate of potash and half a part of charcoal in powder; project this mixture, in small quantities at a time, into a red hot crucible, and repeat this operation three times, adding each time to the residuum new portions of nitrate of potash and charcoal.

The mass resulting from these detonations must then be mixed with one part of black flux, and exposed for the space of an hour in a crucible to a red heat.

The whole is then to be left to cool, the metallic cobalt to be separated, pulverized, mixed with three times its weight of nitrate of potash, and the mixture detonated with the same precautions as mentioned above.

The iron contained in the cobalt will then be strongly oxydated, and the arsenic acidified combines with the potash. The mass pulverized is to be repeatedly lixiviated and filtered: in this manner the arseniate of potash formed will be separated from the insoluble residue which contains the cobalt.

This residue is then to be treated with nitric acid, which dissolves the cobalt without attacking the iron which is found at its *maximum* of oxydation.

The solution is then to be evaporated to dryness, the residue re-dissolved in nitrous acid, and the liquor filtered, in order to separate the last portions of oxyd of iron which might have escaped in the first operation.

All that remains to be done after this is, to decompose the nitrate of cobalt by potash, to wash the precipitate, and to effect its reduction by means of heat.

Extract

Extract of a Memoir by Messrs. FOURCROY *and* VAUQUELIN, *upon the Discovery of a new inflammable and detonating Substance, formed by the Action of the Nitric Acid upon Indigo and the Animal Matters.*

From the ANNALES DES CHIMIE.

THE treatment of animal and vegetable matters by the nitric acid has, it is well known, given rise to a number of important discoveries. The disengagement of a part of the azote of the animal compounds, their conversion into oxalic acid, observed by Berthollet, the formation of ammoniac and of the prussic acid, discovered by Fourcroy, will fix a brilliant epocha in the history of chemical science. The changes which the organic compounds undergo from the action of the nitric acid, from whence result nine or ten substances, themselves compounded, are themselves so multiplied, so various, that they excite the astonishment of chemists, and afford ground to consider this action of the nitric acid as a prolific source of interesting results, a source which, as will be seen from the discovery of two matters almost entirely unknown till the present day, which form the subject of this memoir, is still far from being exhausted.

The most remarkable of these two matters is formed by the ebullition of the nitric acid upon the azoted animal or vegetable substances. It is yellow, extremely bitter, and distinguished by its property of burning and detonating with violence by the aid of a moderate heat.

M. Haussmann, in a memoir inserted in the Journal de Physique (March 1788), when he treated indigo with the acids, appears to have seen this substance. Though he has confounded it with the oxalic acid, he indicates

very clearly its properties, its bitterness, its yellow colour, its solubility and precipitation by alkali; but its principal property, that of inflaming and detonating, consequently its essential and peculiar nature, has entirely escaped his observation.

The matter which Welter has termed the bitter *(l'amer)*, wherein he discovered the detonating property, but which he attributes to the presence of the nitrate of potash, appears to be the same substance.

The most convenient method of obtaining the substance in question, is to boil four parts of nitric acid, of eighteen or twenty degrees, with one part of pulverized indigo, *(guatinata)*, till its colour is destroyed, the acid has assumed a yellow hue, and there remains at the surface of the liquor only a thin layer of a resinous matter, which becomes fixed by cooling, and which is to be separated; to evaporate the solution to the consistence of honey, to dissolve the residue in hot water, to filtrate and to pour into the liquor a solution of potash of commerce, which forms in it small yellow crystals of a circular form: this is the inflammable matter.

The resin which has been separated is converted entirely by a new addition of nitric acid into yellow detonating matter.

If we stop the operation before the period indicated instead of the detonating matter, we obtain another yellow and crystalline substance, but which is more soluble in water, and sublimes in the form of white needles.

This substance presents all the properties of benzoic acid altered by resin. In case the operation is continued it is probable that the benzoic acid is decomposed or volatilized.

The

The orange colour which the detonating matter effects, its bitter taste, its solubility in boiling water, in alcohol, and especially in nitric acid, the very intense blood-red colour which it receives from the alkalies, and which it communicates to the precipitate formed by the sulphate of iron, the tenacity with which it adheres to the benzoic acid which is formed with it by the action of the nitric acid upon the indigo, finally, its property of detonating violently with a bright purplish flash, when wrapped in a piece of paper, and struck with a hammer, are characters which sufficiently distinguish this matter from all other known substances.

The celebrated authors of this memoir have convinced themselves, that the detonating property of their new-discovered substance, was owing neither to the presence of nitric acid nor to that of ammoniac. Concentrated sulphuric acid disengaged from it no acid vapour, caustic potash no ammoniacal vapour. They find more reason to believe that potash acts some part in this detonation: acids in which this matter has been digested contain some traces of salts, with a base of potash; deprived of alkali, the matter is more soluble in water, and crystallizes in oblong laminæ of a yellow colour, of a bitter taste, presenting acid characters; these crystals when wetted with liquid potash, recover their detonating property. The effect which the potash produces appears to be confined to rendering this matter more fixed, favouring the accumulation of heat, and consequently determining the combustion of the elements that form it, of the carbon, of the hydrogene, and perhaps of the azote, by the aid of the oxygene which is likewise contained in it.

Indigo is not the only substance which furnishes the detonating matter: the muscular fibre, treated by the nitric

nitric acid, presents the same phenomena, and it is probable that silk, wool, and the other animal and vegetable matters containing azote, may yield it likewise.

The experiments of Messrs. Fourcroy and Vauquelin present two interesting facts. It results from them, 1, that the benzoic acid can be formed from its constituent principles, which was hitherto not known; 2, that the animal and vegetable substances containing azote, through the agency of the nitric acid, which separates from them carbon, hydrogene, and azote, give rise to a substance super-saturated with oxygene, which communicates to it the detonating property. This substance, which the authors have examined with attention, appears to them to be a super-oxygenated hydrocarburet of azote.

List of Patents for Inventions, &c.

(Continued from Page 80.)

JOHN HARTOP, of Brightside, in the parish of Sheffield, in the county of York, Iron-master; for certain improvements in the method of preparing malleable iron for the purpose of making the same into bars, sheets, and slit rods, and manufacturing the same also into hoop-iron; and for certain improvements in the method of preparing all other malleable metals.
Dated November 7, 1805.

JOHN TROTTER, of Soho-square, in the county of Middlesex, Esquire; for a rotary-engine, for applying the powers of fluids as first movers.
Dated November 14, 1805.

WILLIAM

WILLIAM MILTON, Vicar of Heckfield, in the county of Southampton, M. A.; for a mode of rendering carriages in general, but particularly stage-coaches, more safe than at present, and various other improvements upon such carriages. Dated November 16, 1805.

JOHN CURR, of Sheffield Park, in the parish of Sheffield, in the county of York, Gentleman; for a method, different from any that has hitherto been invented or known, of laying a rope, or, in other words, of twisting and forming the strands together that compose the round rope. Dated November 16, 1805.

ANDREW FLINT, of Gee-street, Goswell-street, in the County of Middlesex, Millwright; for a machine upon an improved construction, which may be used as a steam-engine. Dated November 16, 1805.

JOHN DE LAFONS, of Threadneedle-street, in the city of London, Watch-maker; for a marine alarum chronometer, for ascertaining the time of a ship's log line running out, the time of the watches on ship-board, and many other useful purposes.
Dated November 19, 1805.

GEORGE WYKE, of Winsley, in the county of Wilts, Esquire; for a method of working pumps of various descriptions by machinery, whereby much manual labour will be spared. Dated November 19, 1805.

WILLIAM POCOCK, of the parish of St. Paul, Covent-garden, in the county of Middlesex, Cabinet-maker and Upholsterer; for improvements on tables for dining, and other use. Dated November 19, 1805.

ARCHIBALD Earl of DUNDONALD; for certain improvements in machinery or mill-spinning, for the spinning of cotton, wool, silk, hemp, and flax, and substi-

tutes

tutes for hemp and flax; communicated to him by THOMAS NELSON, late of Lambeth, Engineer, deceased.
Dated November 19, 1805.

RICHARD LUMBERT, of Wick Rissington, in the county of Gloucester, Gentleman; for an improved thrashing-machine; and also an improved portable windlass, to be worked by men, or one or more horses, particularly useful for drawing or hauling.
Dated November 23, 1805.

RICHARD BROWN, of the parish of St. Botolph, Bishopsgate, in the City of London, Cabinet-maker; for certain improvements in the construction of several parts of tables, and of various other articles of household furniture, which stand upon, or are supported by, legs or feet.
Dated November 26, 1805.

JAMES INGRAM, of Castle-street, in the city of Bristol, Grocer; for a method of manufacturing powder sugar from raw sugar alone, and from syrup of sugar alone, and from the mixtures of raw sugar and syrup of sugar.
Dated November 26, 1805.

SAMUEL AMOSS, of Red Lion-place, in the parish of St. Sepulchre, in the city of London, China Enameller; for certain improved methods of preparing various enamel colours, and of applying the same so prepared to the ornamenting useful vessels of glass.
Dated November 26, 1805.

THE

REPERTORY

OF

ARTS, MANUFACTURES,

AND

AGRICULTURE.

No. XLV. SECOND SERIES. Feb. 1806.

Specification of the Patent granted to ALEXANDER WILSON, *of Titchborne-street, Piccadilly, in the County of Middlesex, Gun-maker; for certain Improvements applicable to Shot-Belts, Powder-Flasks, and Fire-Arms of all Descriptions.* Dated July 3, 1805.

With a Plate.

TO all to whom these presents shall come, &c. NOW KNOW YE, that in compliance with the said proviso, I the said Alexander Wilson, do hereby describe and ascertain the nature of my said invention, and the manner in which the same is to be performed, by the drawings in the margin of these presents, and as follows; that is to say:

Fig. 1 (Plate VI.) an interior-view of the breech. A, the chamber containing the charge, being nearly the shape of an egg, cupped out at the top with a cherry, the size of the bore, which prevents the bullet, shot, or wadding, from compressing the powder, and consequently adds to its

explosion and velocity, is much easier cleaned, and the barrel is not so liable to lead or lose in shooting. B, the interior metal touch-hole, projecting into the chamber in the form of a cone, prevents the shot from lodging so as to make the gun miss or hang fire. C, the bottom of the anti-chamber, which passes beyond the interior or inside touch-hole about one-sixteenth of an inch.

Fig. 2, an exterior or outside view of the lock-side of the breech. D, the extremity of the anti-chamber; and its diameter upon the lock-side of the breech, forming an interior cone from C to D (Figs. 1 and 2.) which contains the priming always loose; and if it should at any time get damp, can easily be removed by the picker Fig. 3, and fresh priming introduced. The exterior part, or lock-sides of the breeching, being sloped in an obtuse angular direction towards the top of pan and breech, as represented in Fig. 11, brings the locks of a double gun nearer together, which renders it more convenient to cock; the left lock conducts the eye quicker to the object, and makes the priming in the pan tend more to the anti-chamber.

Gun-Locks.

Fig. 4, the under jaw of the cock being sloped towards the outside, the upper jaw, corresponding with the under, holds the flint in an oblique direction, and, striking the hammer in that direction, (the hammer-face being also oblique, or twisted towards the outside of the lock,) gives the flint more friction, increases the quantity of fire, and throws it close to the amber or touch-hole. The webs of the pan are rounded, which prevents the powder from lodging, so as to disjoint the hammer, and the pan is easier cleaned, and not liable to lose the priming.

Fig.

Fig. 5, an iron or metal rest for a gun, being well calculated for a rifle or other gun, it being forged solid upon the second pipe, or any other convenient place, may be either brazed on, screwed on, fixed with soft solder, or made to act with a swivel or joint. A metal or iron socket being fixed upon the end of the rammer, the rest on the pipe is introduced into the socket. The head of the hammer being large and chequered, to adhere to the small clothes, is placed upon the thigh, near the groin, either standing or kneeling, which supports the gun with such ease, that a very heavy one can be elevated or depressed at pleasure; or the head of the rammer may be placed upon the ground, the rest being previously fixed on the socket.

Shot-Belt.

The head of the shot-belt (Fig. 6,) is made to contain more or less quantities of shot, as occasion may require; is filled with accuracy, without loss of time or hazard in filling the shot, and always contains a charge ready to load with.

Fig. 6, *a a a a*, the extremity of the exterior or outward measure that contains the charge, and which can be raised or lowered at pleasure on the inner tube, and fixed at proper distances by the screw *c c*, according to the quantity or charge required. A, the cover, into which a piece of steel is screwed, and afterwards hardened and tempered, to prevent the friction of the spring from wearing it.

Fig. 6, B, the top or extremity of interior or inner tube. C, the lower end, or extremity which is screwed into the Plate VII, marked H. D, the hoop, that is soldered or cast on the exterior tube, Fig. 9. C C, the slit and screw, by which the exterior tube is elevated or

depressed. F F F, the perpendicular spring and screw that fastens it, which prevents the cover from opening to spill the shot.

Fig. 7, H, the plate to which the interior tube is screwed. I I I, the spring and lever by which the flap acts that admits the shot into the charger.

Fig. 8, the flap which admits or cuts off the communication of the shot, is sloped nearly from the centre to the edge; which edge is rounded, to prevent the flap catching or cutting the shot; the interior or inner tube is also rounded or sloped at the bottom edge.

Fig. 9, the brass hoop, which is soldered to the bottom of the plate Fig. 7, to which the shot-bag is fastened, and which is about one inch in depth, and grooved, in order to make the leather adhere to it, and is widest at the bottom, to prevent it from slipping.

Fig 10, an interior view of the cover of the shot-belt, which acts upon the perpendicular spring F F F, shewn in Fig. 6. The spring acts upon the part marked G.

Fig. 11, a view of the back end of the plugs or breeching of a double gun, which are sloped more or less, in an obtuse angular direction towards the top of the breeching, to bring the locks nearer, or to keep them at a greater distance from each other, as occasion may require.

Fig. 12, the tool, made of thin sheet steel, or other metal, with which the breeching or plugs are sloped.

The advantages to be derived from my improved breeching or plugs are, that they have an interior metal touch-hole, placed in a peculiar manner, behind the centre of the charge, projecting into the chamber, in the form of a cone, and thereby preventing the shot from lodging,

Fig. 1.

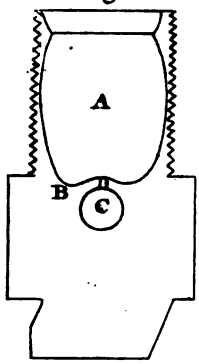


Fig. 2.

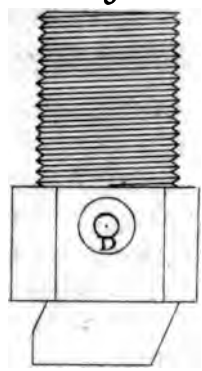


Fig. 4.

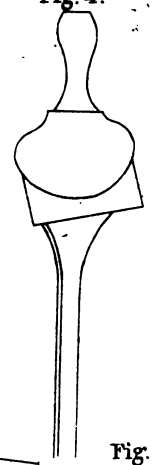


Fig. 6.

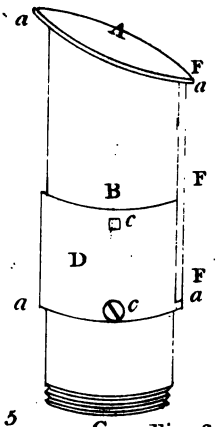


Fig. 12.

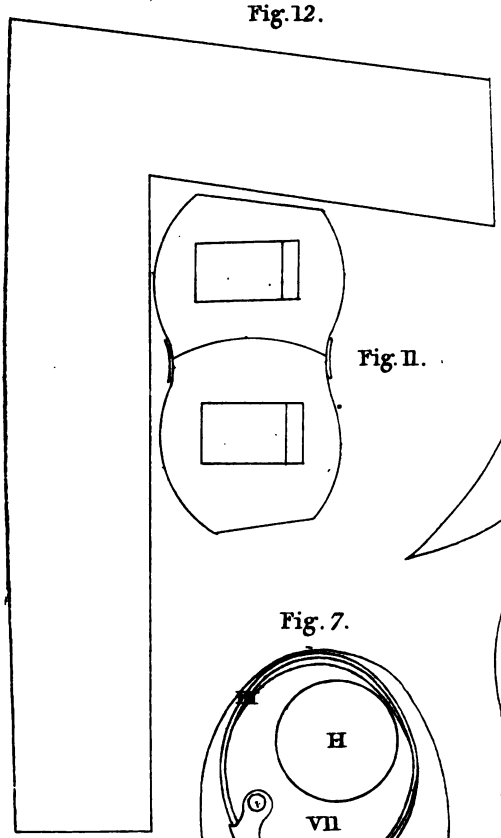


Fig. 5

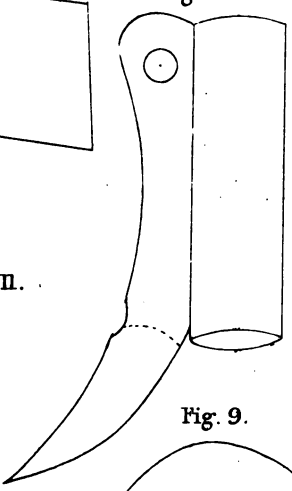


Fig. 3.

Fig. 11.

Fig. 9.

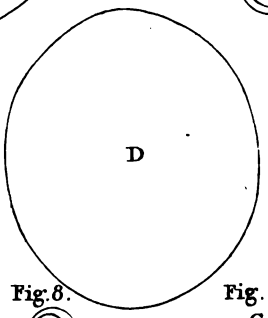


Fig. 7.

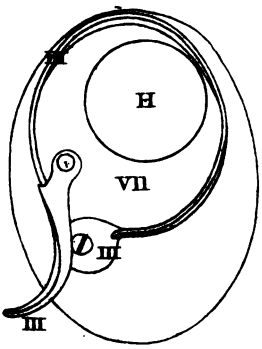


Fig. 8.

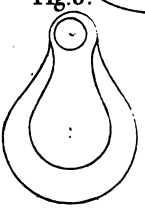
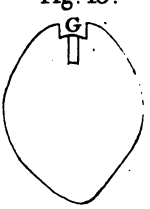


Fig. 10.



lodging, so as to make the gun miss or hang fire; the touch-hole also communicates with the powder in the pan through the anti-chamber, which is so constructed, that in damp weather the gun cannot miss or hang fire. The inside of the breeching which contains the charge is nearly of the shape of an egg, and so constructed as to permit the powder to remain always loose. It is easier cleaned, and has the advantage of lighting the powder much quicker, and in a more perfect manner than any breeching heretofore used. The locks of a double gun are nearer together than any hitherto made, which renders it more convenient to cock the left lock. The exterior part of the breeching being sloped in an obtuse angular direction, conducts the eye quicker to the object, and makes the priming in the pan tend more to the anti-chamber. Guns on this new principle are much stronger than others: they will throw the shot more regular, with greater velocity, and are not so liable to lead or lose their shooting.

The jaws of the cock hold the flint in an oblique direction, which gives it more friction, increases the quantity of fire, and throws the fire close up to the touch-hole. The web of the pan is rounded, which prevents the powder from lodging, so as to disjoint the hammers. The pan is easier cleaned, the priming always kept dry, and the pan not liable to lose it.

The rest for rifles does not exceed two inches in length and one ounce in weight, may be used standing, or in any other position. A very heavy gun may be elevated or depressed with one hand, at pleasure, and at the same time kept perfectly steady.

The head of the shot-belt is so constructed as to contain various quantities of shot, is filled with accuracy, without loss of time or hazard in spilling, and always contains

contains a charge, as occasion may require, ready to load with. This improvement upon the shot-belt is equally applicable to the powder-flask.

In witness whereof, &c.

OBSERVATIONS BY THE PATENTEE.

It must be understood, from the preceding specification, that my guns have a triple advantage, *viz.* the breeching, locks, and rest. In loading they prime themselves, owing to the close connection of the touch-hole with the anti-chamber, which is an advantage far superior to the guns on the old principle, they being uniformly made with wide exterior touch-holes, which must evidently tend to the diminution of the strength of the charge.

The powder-flask is made to contain the same quantity of powder as the chamber of the gun, which consequently brings the powder into close contact with the wadding or shot, and prevents the air from lodging so as to diminish the effect of the powder.

In loading a double gun on the old principle, pallets of shot are apt to fall into the cylinder hole of the breech of the unloaded barrel, and stop the communication between the powder in the pan and the charge, which is with great difficulty removed. Double guns on my principle are entirely prevented from such inconvenience, by having the interior touch-hole elevated in the bottom of the chamber in a conical form, which wholly prevents the pallets from lodging on it.

Specification of the Patent granted to MATTHEW GREGSON, *of Liverpool, Upholder; for a Method of cleansing Feathers for Beds, and Hair, Wool, Down, and other the natural Covering of Birds and Animals from their Animal Oil, in the most perfect Manner, and in such a Way as to render them more healthful, sweet, and pleasant for Use.*

Dated December 4, 1804.

TO all to whom these presents shall come, &c. NOW KNOW YE, that in compliance with the said proviso, I the said Matthew Gregson do hereby declare, that the nature of my said invention, and the manner in which the same is to be performed, are hereinafter particularly described and ascertained, as follows; that is to say:

First, I place the said feathers, and the hair, wool, down, and other natural covering of birds and animals, or any of them, attached to the skins or hides, or otherwise, in a chamber or vessel, or other proper receptacle, closed on all sides except at that part where the heat, or heated elastic fluid, hereafter to be described, is to be introduced; and also at another part, where the exhalations or vapours of animal oil, and other impurity, are suffered to pass out. And I dispose the said feathers, hair, wool, down, or other natural covering of birds and animals, thinly, lightly, or openly, within the said chamber, vessel, or receptacle, or other proper receptacle, by means of shelves, sticks, or other supports, if needful, in such manner that the said heat, or heated elastic fluid, may be duly applied to all and every part and portion thereof.

Secondly,

Secondly, I do, by the usual and well-known methods, raise steam, or I do heat the common air of the atmosphere, and cause the same to enter through an aperture, and by means of pipes or flues, when needful, into the said chamber or vessel, or receptacle, so as to give sufficient heat to the contents thereof, as aforesaid, in order to evaporate and drive off the offensive animal oil, and render the said contents more sweet and pleasant for use; for which purpose I permit the said steam or heated air to escape again along with the said offensive animal oil, through another aperture in the said chamber, vessel, or receptacle; and I do so manage my fire, and regulate the said apertures, that the heat shall be adequate to the intended purpose, and continued a sufficient time to complete the intended effect.

Thirdly, and lastly, instead of, or along with, the steam or heated atmospheric air aforesaid, I do avail myself, and make use of, such other heated vapours or elastic fluids, or gasses, as may, with economy and advantage, be applied to my said purpose; and more particularly I do use the altered residue, or steam of heated and burned common air, which has passed through, or amongst, certain bodies in combustion, or fuel, which do not give out any taint or smell, or otherwise injure the good qualities of the said feathers, hair, wool, down, or other natural coverings of birds and animals, so intended to be rendered sweet and pleasant for use.

In witness whereof, &c.

Specification

Specification of the Patent granted to STEPHEN CLUBB, *of Colchester, in the County of Essex, Millwright; for an improved Mangle.* Dated September 27, 1805.

With an Engraving.

TO all to whom these presents shall come, &c. NOW KNOW YE, that in compliance with the said proviso, I the said Stephen Clubb do hereby declare, that my said invention of an improved mangle is described and ascertained in and by the drawings and description thereof underneath these presents.

In witness whereof, &c.

Fig. 1, (Plate VII.) A B, are two hollow rollers, which may be both of a size and length, about two feet six inches long each, and about one foot two inches diameter, and they may be made with a flat side to each, as C C, which should consist of full one-fourth or more of the circumference, and an inch and an half deep, or more, from the true circle, and should rise regular from the deepest part of the flat D, to the highest or true circle E; so that by turning the handle F the two flats will meet together at the small roller G, which will give it above three inches room to take out or to put in. The three inches, or thereabouts room, will also make room enough to put the small roller G in when the linen is wound on it; which, when done, and the handle F turned round, it will bring the weight of the upper roller A (which must be sufficiently loaded with heavy materials) upon the linen, or the small roller G. The flat C should be made to open from black line *a* to black line *b*, in order to put in the loading. The centre of the upper roller H, and the centre of the pinion I, should have room to rise and fall three or four

inches, and should be connected by the connecting iron K. The two wheels L M may be both of a size, and about two inches larger in the diameter than the rollers on which they are fixed. The two pinions also which work in them should be both of a size, and about two inches and a half the diameter. The small roller G is fixed in the frame by a button, which turns over the spindle or centre of it at N. On the other side of the frame, the end of the cloth which goes round the linen works up and down when the mangle is at work, as at O. The frame P P may be fastened together with strong screws, or otherwise, into the cross rail Q, which may be ten inches or more wide, and about two inches and a half thick. The two side pieces of the frame P P may be of the same thickness as Q, and about four inches and a half wide: the feet R R, of frame P P, may be two feet long, little more or less. The small roller G, on which the linen is wound, is about three inches in diameter, and is made to take out of the frame on the contrary side of which the pinions work, as at S.

Fig. 2, end view of Fig. 1. C, the flat, D, the depth, E, the circle, or highest part of the flat, H, the centre of the roller. I, the centre of the pinion. K, connecting iron. M, the wheel. O, the cloth. P, the frame. R, the feet.

I make my improved mangle of any other convenient size or dimensions as may be required.

OBSERVATIONS BY THE PATENTEE.

The various excellences which my mangle contain are almost hid under the simplicity of its principle; but I think it may not be amiss to attempt to point out two or three of them.

First—

Fig. 3.

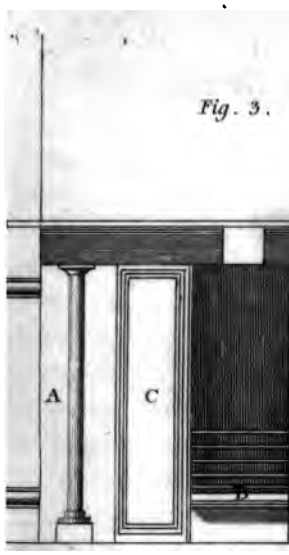


Fig. 5

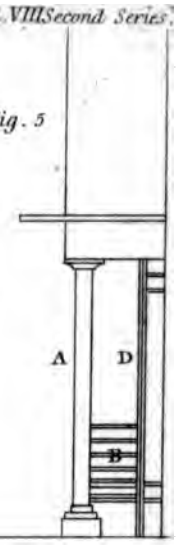


Fig. 6.

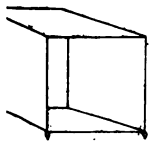


Fig. 4.

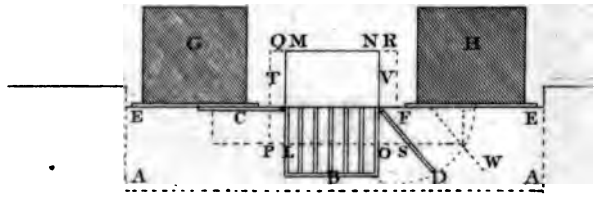


Fig. 1.

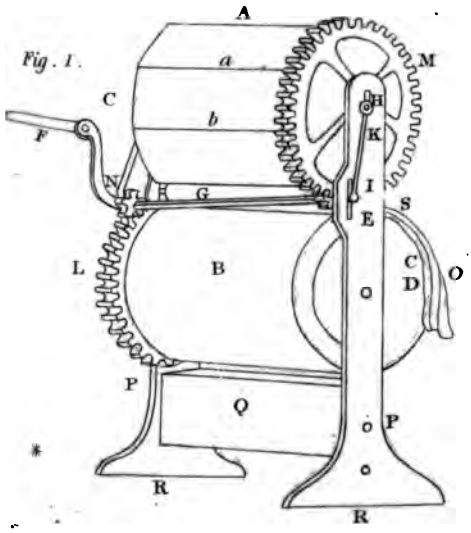
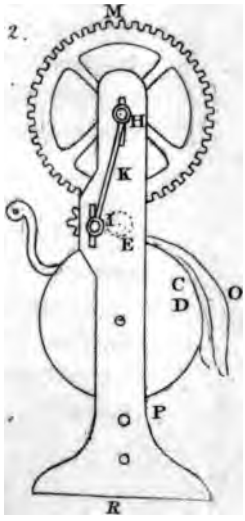


Fig. 2.



First. It is often observed by mechanics, that time and power cannot increase together, for as one increases the other diminishes; but with respect to my mangle, when compared with others, it will be found to gain double motion by the same power; for as the lower part or roller moves by the resisting power of the upper one, there must be double motion.

Secondly. As the weight is constantly upon the linen, from the time it is introduced into the mangle until delivered out, the mangling must certainly be executed faster than if the linen were to be worked off of one roller on to another, as in the mangles of the usual construction.

Thirdly. The weight used in my mangle is little more than half the weight required in any other mangles, consequently a little more than half the labour or power employed in other mangles will be necessary to set mine to work.

Specification of the Patent granted to ALEXANDER BRODIE, *of Carey-street, in the Liberty of the Rolls, in the County of Middlesex, Iron-Master; for his new and improved Form or Method of making Steam-Engine-Boilers, and Steam-Boilers for various other Purposes, and of constructing the Flues for conveying the Heat to the same, whereby the Consumption of Fuel is considerably lessened.* Dated May 31, 1805.

With an Engraving.

TO all to whom these presents shall come, &c. NOW KNOW YE, that I the said Alexander Brodie, in compliance with, and in pursuance of, the said proviso in the said recited letters patent contained, (by this in-

strument in writing, under my hand and seal,) do particularly describe and ascertain the nature of my said invention in the said letters patent mentioned, and in what manner the same is to be performed, and declare the same to be in manner following; that is to say: I make my cast-iron boiler of any size, in form as by the drawing hereunder appears; the cast-iron plates are made with flanges, to any size that may be wanted for the boiler, and put together with rivets or screws, and nuts and paste-board between the plates; and, to prevent the boiler from giving way by force of the steam, there are several wrought-iron stays fixed in the inside of the boiler. The boiler so made, stands upon iron, so that there is scarcely one quarter of an inch but what the fire has its full power over the bottom of it. There are two fire-places, and the flue round is done with brick, according to the height and width that may be required. The top of the boiler is flat, and I cover it with brick.

In witness whereof, &c,

EXPLANATION of SKETCH for CAST-IRON BOILER.

Fig. 1, (Plate VIII.) is a cross section of the boiler. The dotted line from *a* shews the flue that goes under, and has a brick-breadth partition in the middle, that equally divides it.

Fig 2 is a plan of the bottom of boiler, and also looking down upon the flue, as at *a a*, at *c c*, &c. In each sketch are flanges, cast to the plates, for the purpose of rivetting the whole together. At D, or thereabouts, in the lid or top of the boiler is a man-hole seventeen inches diameter.

Specification

Fig. 3.

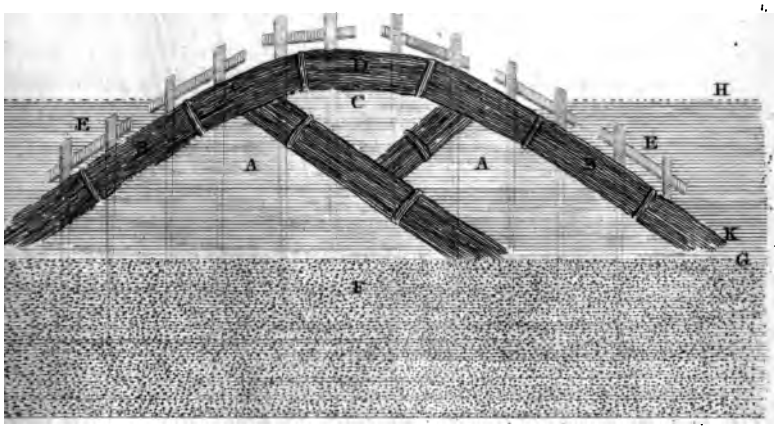


Fig. 1.

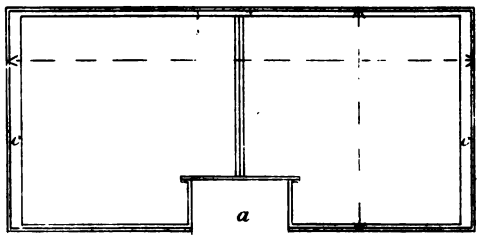
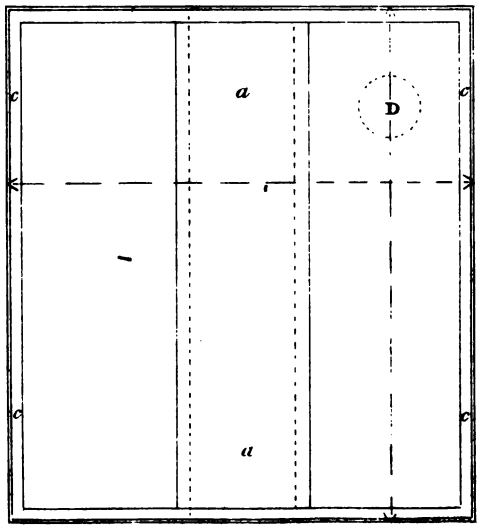


Fig. 2.



Specification of the Patent granted to BARRODALL ROBERT DODD, *of Change-alley, in the City of London, Civil Engineer; for various Improvements in the Construction of Fire-places, and adapting Stoves and Grates thereto.* Dated April 18, 1805.

With a Plate.

TO all to whom these presents shall come, &c. NOW KNOW YE, that in compliance with the said proviso, I the said Barrodall Robert Dodd do hereby declare, that my invention is fully described and ascertained in manner and form following, reference being had to the drawings in the margin of these presents; that is to say:

Fig. 3 (Plate VII.) is an elevation or front-view of the fire-place.

Fig. 4 an horizontal section of the same, immediately below the breast of the chimney.

Fig. 5, a side-view. The same letters refer to the same parts in each figure.

A, columns supporting the chimney.

B, the grate.

C and D reflectors of metal, or other suitable materials.

By the plan, Fig. 4, the manner of constructing the fire-place, the flues, and the situation of the grate, is clearly shewn.

G and H are flues of fire-places, supposed to be in apartments below. I and K plates of iron, of which the sides of the flues in the room may be formed, from the hearth to the breast of the chimney, to save an unnecessary projection of brick-work in the room. L M N O, the throat

throat or opening of the chimney, which may be contracted at pleasure, by plates of iron or registers, in the usual way; but, where a larger aperture may be required, it can be extended as at P Q R S, and a part thereof at each side, as at T V, may be made to lift up at pleasure, in the manner of a register. C represents one of the reflectors, pushed back close against the wall, and D the other half open. In the drawing the reflectors are placed close to the grate, but they should be moveable, or made to slide horizontally in grooves, or on rollers, or otherwise, from E to F, which will permit them to open at different angles, and at various distances from the grate, as shewn at W. They may be fixed on hinges, or otherwise, vertically, to the back of the fire-place; or they may be united to the sides of the stove or grate. The reflectors may be made either of one entire piece, or separated in the centre, or elsewhere, as may be judged to be most convenient; and they might be so constructed as to slide into a recess formed in the wall, or contrived so as to slide out, or turn completely round, by which either side may be presented to the fire; one of which only might be polished, and the other japaned. In some cases it might be useful to make the reflectors without either affixing them to the fire-place or the stove, in the manner of a fire-skreen, to stand upon legs, or otherwise; but there are a variety of modes in which the reflectors might be applied.

The dotted line E A, E A, denotes the space that the brick-work above the columns, forming the chimney, projects over.

Brackets or trusses might be applied, to support the chimney or brick-work, instead of columns, if preferred.

The

The principal intention and advantages of these reflectors are to augment the warmth of the room without increasing the consumption of the fuel. They may be used on the first kindling of a fire, by being drawn forward at right angles, close to the stove or grate, on each side; in which situation they will assist in producing a strong draught, and cause the speedy inflammation of the fuel in the grate. When this is effected, they may be pushed about half way back, in order that the radiant heat from the sides of the fire may impinge against them, and thence be reflected into the room; but when the fire has burnt clear, or no smoke is apprehended, the reflectors should be thrown quite back: in which case the direct calorific rays from both sides of the fire will all pass into the room in the same manner as they now do, from the front only, and thereby raise the temperature of the apartment without adding to the consumption of fuel, and at the same time allow a more extensive view and benefit of the fire than has been obtained by any other mode.

Fig. 6 is an extinguisher, or metal case, so formed as to completely inclose the stove or grate with the fire, which will, when put on, exclude the air, which is so essentially necessary to combustion, and thereby speedily smother, or put out the fire, without removing the heated cinders, creating any dust or trouble whatever; and it will also save the coals in the grate or stove at the time of extinguishing the fire.

In witness whereof, &c.

On an artificial Substance which possesses the principal characteristic Properties of Tannin.

By CHARLES HATCHETT, *Esq. F. R. S.*

From the PHILOSOPHICAL TRANSACTIONS of the ROYAL SOCIETY.

§ I.

THE discovery of the principle on which the effects of tannin essentially depend, may be partly attributed to Mr. Deyeux, who obtained a substance from galls which he considered as a species of resin *, but which was afterwards proved by Mr. Seguin to be that which renders the skins of animals insoluble in water, and imputrescible, and thus to be the principle by which they are converted into leather †.

The chief characteristic property of this substance was ascertained by Mr. Seguin to be that of precipitating gelatine or glue from water in a state of insolubility; and as it was evidently different from any vegetable substance hitherto discovered, he gave it the name of tannin.

This discovery of Mr. Seguin at once unveiled the theory of the art; an easy and certain method was afforded by which tannin could be detected, and its relative quantity in different substances be determined, whilst the nature and properties of this newly-discovered vegetable principle could be subjected to accurate investigation.

* Mémoire sur la Noix de Galle, par M. Deyeux; Annales de Chimie, tome XVII. p. 23.

† Ibid. tome XX. p. 15.

The

The former has derived elucidation from the experiments of Mr. Biggin *, and much has been contributed in every respect by Mr. Proust †, but the subject has received the greatest extension, and some of the most valuable additions, from the ingenious labours of Mr. Davy, particularly the discovery of the important fact, that catechu or terra japonica consists principally of tannin ‡.

The united results of the experiments made by these and other eminent chemists, appear therefore to have fully established, that tannin is a peculiar substance or principle which is naturally formed, and exists in a great number of vegetable bodies, such as oak bark, galls, sumach, catechu, &c. &c. commonly accompanied by extract, gallic acid, and mucilage.

But no one has hitherto supposed that it could be produced by art, unless a fact noticed by Mr. Chenevix may be considered as some indication of it.

Mr. Chenevix observed, that a decoction of coffee-berries did not precipitate gelatine unless they had been previously roasted; so that tannin had in this case either been formed, or had been developed, from the other vegetable principles by the effects of heat §.

Some recent experiments have however convinced me, that a substance possessing the chief characteristic properties of tannin may be formed by very simple means, not only from vegetable, but even from mineral and animal substances.

* Phil. Trans. 1799, p. 259; and vol. XII. of the first series of this work, p. 25.

† Annales de Chimie, Tome XXV. p. 225. Ibid. Tome XLI. p. 331. Ibid. XLII. p. 89; and vol. XI. p. 59, first series, and vol. I. p. 145, new series of this work.

‡ Phil. Trans. 1803, p. 233; and vol. III. p. 165, new series of this work.

§ Nicholson's Journal for 1802, vol. II. p. 114.

§ II.

In the course of my experiments on lac, and on some of the resins, I had occasion to notice the powerful effects produced on them by nitric acid; and I have since observed, that by long digestion almost every species of resin is dissolved, and is so completely changed, that water does not cause any precipitation, and that by evaporation a deep yellow viscid substance is obtained, which is equally soluble in water and in alcohol, so that the resinous characters are obliterated.

When I afterwards had discovered a natural substance, which was composed partly of a resin similar to that of recent vegetables, and partly of asphaltum *, I was induced to extend the experiments already mentioned to the bitumens, in the hope of obtaining some characteristic properties by which the probable original identity of these bodies with vegetable substances might be farther corroborated. In this respect I succeeded, in some measure better than I expected; but I observed a very material difference between the solutions of the resins and those of many of the bitumens, such, for instance, as asphaltum and jet. The first effect of nitric acid, during long digestion with these substances, was to form a very dark brown solution, whilst a deep yellow or orange-coloured mass separated, which by subsequent digestion in another portion of nitric acid was completely dissolved, and by evaporation was converted into a yellow viscid substance, equally soluble in water and in alcohol, so as to perfectly resemble that which, by similar means, had been obtained from the resins, excepting that when burned it emitted an odour somewhat resembling that of the fat oils.

* Phil. Trans. 1804, p. 385.

It

It therefore appeared to me, that the first or dark brown solution had been formed by the action of the nitric acid on the *uncombined* carbonaceous part of the bitumens, or that by which they are rendered black; and that the deep yellow portion which was separated was that which constituted the real or essential part of these bituminous substances. This opinion was confirmed by some experiments which I purposely made upon amber; and having every reason therefore to believe, that the dark brown solution obtained from asphaltum and jet was in fact a solution of coal, I repeated the experiments on several varieties of the pit or mineral coal, from all which I obtained the dark brown solution in great abundance; but those coals which contained little or no bitumen did not yield the deep yellow substance which has been mentioned.

In each experiment I employed 100 grains of the coal, which I digested in an open matrass, with one ounce of nitric acid diluted with two ounces of water. (The specific gravity of the acid was 1.40.)

After the vessel had been placed in a sand-bath, and as soon as it became warm, a considerable effervescence, attended with much nitrous gas, was produced: after about two days I commonly added a second, and sometimes a third ounce of the acid, and continued the digestion during five or six days, or until the whole, or nearly the whole, was dissolved, excepting in those cases when the deep yellow substance was formed; for this I constantly separated.

The next experiment was made upon charcoal, which was more readily dissolved than the preceding substances, without leaving any residuum; the solution was perfect, and the colour was reddish-brown *.

* The solubility of charcoal in nitric acid, and some of its properties

Having thus, by means of nitric acid, obtained solutions from asphaltum, from jet, from several of the pit-coals, and from charcoal, I evaporated them to dryness in separate vessels, taking care in the latter part of the process to evaporate very gradually, so as completely to expel the remainder of the acid without burning the residuum; this, in every case, proved to be a brown glossy substance, which exhibited a resinous fracture.

The chemical properties of these residua were as follows:

1. They were speedily dissolved by cold water and by alcohol.

2. Their flavour was highly astringent.

3. Exposed to heat, they smoked but little, swelled much, and afforded a bulky coal.

4. Their solutions in water reddened litmus-paper.

5. The same solutions copiously precipitated the metallic salts, especially muriate of tin, acetite of lead, and oxysulphate of iron. The colour of these precipitates was commonly brown, inclining to that of chocolate, excepting the tin, which was blackish-grey.

6. They precipitated gold from its solution in the metallic state.

7. They also precipitated the earthy salts, such as the nitrates of lime, barytes, &c. &c.

8. The fixed alkalis, as well as ammonia, when first added to these solutions, only deepened the colour, but after some hours rendered them turbid.

when thus dissolved, have been noticed by Professor Lichtenstein in Crell's Chemical Annals, 1786, by Mr. Lowitz, (Crell's Chem. Journal, translated into English, Vol. II. p. 255); and by Mr. Jameson, in his Outline of the Mineralogy of the Shetland Islands, &c. 8vo. edit. p. 167.

9. Glue

9. Glue or isinglass was immediately precipitated by these solutions from water, and the precipitates were more or less brown, according to the strength of the solutions. The precipitates were also insoluble in cold and in boiling water, so that in their essential properties they proved similar to those formed by the varieties of tannin hitherto known, with the difference, that this factitious substance appeared to be exempt from gallic acid and mucilage, which commonly accompany the varieties of tannin, and which occasion modifications in the colour and appearance of some of their precipitates.

Having thus had the satisfaction to discover, that a product resembling tannin could be formed by such a simple method, not only from vegetable but also from mineral coal, I was induced to examine how far the same might be extended to animal coal, and I therefore reduced a portion of isinglass to that state in a close vessel, and having rubbed it into fine powder, I digested it with nitric acid in the manner which has been described. At first the acid did not appear to act upon it, but at length it was slowly dissolved, excepting a small quantity, which however was in every respect unchanged; and here we may remark, that as animal coal is incinerated with much more difficulty than vegetable coal or charcoal, so was the same difference to be observed, when oxygen was presented to these bodies in the humid way.

The solution resembled those which have been described, excepting that the brown colour was more intense. It was evaporated to dryness, and was then dissolved in distilled water; after which the solution, being examined by the re-agents which had been employed in the former experiments, was found to produce similar effects,

effects, excepting some difference in the colour of the precipitates.

I next added some of the liquid to a solution of isinglass, and obtained a copious precipitate. Thus it is evident, that a tanning substance may be formed from animal as well as from vegetable and mineral coal; and it is not a little curious, that this enables us to assert as a matter of fact, although not of economy, that one portion of the skin of an animal may be employed to convert the other into leather.

In the course of these experiments, I also subjected coak to the action of nitric acid, and obtained a product which resembled that which had been afforded by pit-coal; but in this case (as might be expected) there was not any appearance of the deep yellow substance which has so often been mentioned.

These experiments therefore prove, that a tanning substance may be artificially formed by exposing carbon to the action of nitric acid; and, it also appears, that this is best effected when the carbon is uncombined with any other substance excepting oxygen. The following experiments seem to corroborate this opinion.

1. A piece of Bovey coal, which had perfectly the appearance of half-charred wood, was reduced to powder, and was digested with nitric acid until the whole was dissolved.

The colour of the solution was deep yellow; and, by evaporation, a yellow viscid mass was obtained, which was dissolved in distilled water. This solution was then examined by various re-agents, and particularly by gelatine, but not any vestige of tanning matter could be discovered, and the predominant substance appeared to be oxalic acid.

2. Another

2. Another piece of Bovey coal, which had less of the characters of wood, and was more perfectly carbonized, was treated in the way which has been described; the solution was brown, and, unlike the former, afforded a considerable precipitate with gelatine.

3. A portion of the first sott of Bovey coal was exposed to a red heat in a close vessel, and was then reduced to powder, and digested with nitric acid: here a remarkable difference was to be observed, for nearly the whole was thus converted into the tanning substance.

4. A coal from Sussex, extremely like the second sort of Bovey coal, also afforded the same product.

5. A piece of the Surturbrand, from Iceland, yielded a similar result.

6. Some deal saw-dust was digested with the nitric acid until it was completely dissolved; by evaporation a yellow viscid mass was obtained, the solution of which in water afforded results like those of the first experiment on the Bovey coal, for oxalic acid was found in it, but not any of the tanning substance.

7. Another portion of the same deal saw-dust was converted into charcoal in a close vessel; the charcoal was then treated in the manner already described, and was thereby formed into a liquid which copiously precipitated gelatine.

8. Having previously ascertained that teak wood does not contain gallic acid nor tannin, I reduced some of it into charcoal, which was afterwards readily converted into the substance above mentioned.

In these experiments the deal and the teak wood had been reduced to the state of coal, as usual, by fire; but as this does not appear to have been the means generally employed by Nature to convert organized substance into the varieties of mineral coal, I for a considerable time previous

previous to the discovery of the artificial tanning product, had been employed in a series of experiments on the slow carbonization of a great number of vegetable substances by the humid way.

The agent which I most commonly used to produce this effect was sulphuric acid occasionally diluted; and although many of the processes were extremely unpleasant and tedious, yet I have not any reason to regret the time which has been thus employed. The subject, however, I foresee, will branch out into several details, none of which as yet I can regard as sufficiently completed to merit the honour of being submitted to this learned Society; but I am in a manner almost compelled in the present case to anticipate a few of the experiments, with their results, because they are intimately connected with the subject now under consideration.

In these experiments, I have observed that concentrated sulphuric acid, when poured upon any of the resinous substances reduced to powder, dissolved them in a few minutes; at this period the solution was transparent, commonly of a yellowish-brown colour, and of the consistency of a viscid oil. But if, after this, the vessel was placed on a sand-bath, the colour of the solution became progressively darker, sulphureous gas was evolved, and at length the whole appeared like a very thick liquid of an intense black. I purposely, for the present, pass over many phenomena, some of which are peculiar to the different substances when thus treated, whilst others are general, and may be referred to those attendant on etherification, for my intention here is only to notice, in a concise manner, such as immediately tend to elucidate the subject of this paper.

When concentrated sulphuric acid is poured on the common turpentine of the shops, it almost immediately dissolves

dissolves it like the solid resins; and if a portion of this solution be poured into cold water, the turpentine is precipitated in the solid brittle state of common yellow resin. But if a second portion of the same solution, after the lapse of an hour or more, be in like manner poured into cold water, the resin thus formed is not yellow but dark brown; and if four or five hours are suffered to elapse before a third portion is poured into water, the resin is found to be completely black. After this, supposing the digestion to be carried on during several days, or until there is no longer any production of sulphureous gas, the turpentine or resin will be found converted into a black porous coal, which, if the operation has been properly conducted, does not contain any resin, although a substance may frequently be separated by digestion in alcohol, which I shall soon have occasion to notice.

When common resin was thus treated, I obtained about 43 *per cent.* of the coal, which, after being exposed to a red heat in a loosely covered platina crucible, still amounted to 30 *per cent.* and by the slowness of its combustion, and other circumstances which need not here be related, approached very nearly to the characters of some of the mineral coals *.

The effects produced by sulphuric acid upon turpentine and resin are manifestly caused by the union of the two constituent principles of the latter (namely, hydrogen and carbon) with part of the oxygen of the former,

* The difference of the quantity of carbon, which may be obtained in the state of coal from resinous substances by the humid and by the dry way, is very considerable; we may take common resin as an example, for when 100 grains were exposed to simple distillation in a small glass retort placed over an open charcoal fire, the residuum of coal only amounted to three-fourths of a grain.

so that sulphureous acid, water, and coal are produced. I therefore availed myself of this process, by which coal could be progressively formed whilst the original substance was gradually decomposed, to make the following experiment.

A quantity of common turpentine was treated with sulphuric acid in the way which has been described, and different portions of the solution being poured at different periods into water, whilst the remainder was digested during several days, I thus obtained, from the same original substance, yellow resin, brown resin, black resin, and coal. I then digested a portion of each of these, as well as some of the turpentine, in separate vessels with nitric acid until they were completely dissolved, and afterwards reduced them to dryness. The different residua varied in colour from yellow to dark brown, corresponding to the substances which had been employed. These were then dissolved in distilled water, and were examined by solution of isinglass and other reagents.

1. The solution of the residuum of turpentine was pale straw colour, and did not precipitate gelatine.

2. That of yellow resin resembled the former in every respect.

3. That of the brown resin was of a deep yellow, but in other particulars resembled the above.

4. That of the black resin, on the contrary, yielded a considerable portion of the tanning substance,—and

5. That of the coal afforded it in abundance.

Hence it appears, that these different modifications of turpentine yielded the tanning substance only in proportion to the quantity of their original carbon, which,

by

by oxidizement, had been progressively converted into coal *.

Other substances, when reduced into coal in the humid way, were in like manner formed into the tanning substance by nitric acid. In fact, I found this to be the constant result; and, amongst the many substances which were examined, I shall mention various kinds of wood, copal, amber, and wax, all of which, when reduced to coal by sulphuric acid, yielded similar products, by subsequent treatment with nitric acid.

But this substance may likewise be artificially produced without the help of nitric acid, although in a less proportion, as well as with some slight variations in its characteristic properties; for, as I have already observed, when any of the resins or gum resins (such as common resin, elemi, asa fœtida, &c.) have been long digested with sulphuric acid so as to assume the appearance and general characters of coal, if afterwards they are digested with alcohol, a portion is dissolved, and a dark brown solution is formed, which, by evaporation, yields a mass soluble in water as well as in alcohol, and which precipitates gelatine, acetite of lead, and muriate of tin, but produces only a very slight effect on oxysulphate of iron. This substance, therefore, which may thus be separated by alcohol from the coal formed from resinous bodies by sulphuric acid, evidently contains some of the tanning matter, which has been produced during the carbonization of those substances.

A natural process, very similar to this, I much suspect takes place in some cases where peat is formed; I say, in

* Some late experiments have, however, convinced me that carbon need not be absolutely converted into coal in order to produce the artificial tanning substance; but this will be more fully explained in a subsequent paper.

some cases, because the production of tanning matter does not seem to be absolutely a necessary consequence attendant on the formation of peat; for in many places where the latter abounds the former cannot be detected, whilst in others it is very abundant, and acts powerfully on animal bodies which have accidentally been exposed to its effects.

There are many facts of this kind upon record, such as the account of the bodies of the man and woman preserved in the moors near the woodlands in Derbyshire, and also of the woman found in the morass at Axholm, in Lincolnshire *. Now I am much inclined to believe, that the tanning substance which so much abounds in these and some other peat moors, did not originally exist in the vegetable substances from which the peat has been produced, but that it has been and continues to be progressively formed (under certain favourable circumstances) during the gradual carbonization and conversion of the vegetable matter into peat.

§ III.

In most of the former papers which I have had the honour to lay before the Royal Society, I have, for greater perspicuity, generally concluded with a recapitulation of the contents, but, in the present case, this appears to be superfluous, as the whole may be concentrated into one simple fact, namely, that a substance very analogous to tannin, which has hitherto been considered as one of the proximate principles of vegetables, may at any time be produced, by exposing carbonaceous substances, whether vegetable, animal, or mineral, to the action of nitric acid.

* Phil. Trans. vol. XXXVIII. p. 413. Ibid. vol. XLIV. p. 571.

Since

Since the preceding experiments were made, I have farther proved the efficacy of this substance by actual practice, and have converted skin into leather by means of materials which, to professional men, must appear extraordinary, such as deal saw-dust, asphaltum, common turpentine, pit-coal, wax candle, and a piece of the same sort of skin.

Allowing, therefore, that the production of this substance must for the present be principally regarded only as a curious chemical fact, not altogether unimportant, yet, as the principle on which it is founded appears to be developed, we may hope that a more economical process will be discovered, so that every tanner may be enabled to prepare his leather even from the refuse of his present materials.

The organized bodies and their products have only of late years much attracted the attention of chemists, many of whom, even at this time, (although the modes of chemical examination have been so much improved,) seem disgusted, and deterred by the Proteus-like changes which take place whenever these substances are subjected to experiment.

But these variable and endless alterations of their properties seem rather calculated to operate as incitements to investigation; for, by the accumulation of facts resulting from the changes produced in these bodies by disuniting and by recombining their elementary principles, not only will chemistry, as a science, become farther illuminated and extended, but it will, as it has hitherto done, render great and essential services to the arts and manufactures.

Remarks

Remarks on sundry important Uses of the Potatoe.

From LETTERS and PAPERS of the BATH and WEST of ENGLAND SOCIETY.

THE potatoe has, though deservedly, occupied so much of the attention of different writers, that it may seem almost necessary to bring forward some new and important discoveries concerning it, if we attempt to say more on its qualities. It is not, however, a singular opinion, that so important is this vegetable, and so applicable to economical uses, as human food; that it will remain for posterity fully to appreciate its positive and comparative value. But as no new and promising experiment, however imperfectly conducted, should be suffered to escape general notice, it will be acceptable to our readers to receive a general statement of certain trials made by a very respectable British merchant, with a view to ascertain the value of the potatoe for sea provision and other stores. His diffidence about having done justice to the subject, which he doubts of finding leisure to prosecute, prevents his allowing his name to appear as to a finished essay of his own; but certain statements are deemed too important to be lost, as they may lead to farther discoveries and facts. The statements then are in substance as follow:

"The ease with which this root is prepared by boiling and for immediate consumption, either in its separate form, or mixed in bread; the little trouble there is in preserving it through the winter months; and the short period between the time of planting and the return of the crop; have most probably been the causes why less pains have been taken to find out cheap methods of preserving

preserving potatoes, as a store for future sustenance, than would otherwise have been the case.

"The large quantity of potatoes produced in the last season, and the reputed scarcity of bread-corn, induced me a few weeks since to make some small experiments on the means of drying potatoes, either in substance or in flour; either for future consumption at home, or for the supply of our seamen on long voyages.

"The ease with which I found this might be done, and the probable benefit which I think may be derived to the public from a farther pursuit of the subject, induces me to submit to the inspection of the Society a small quantity of the flour of potatoe sent herewith.

"The potatoes were boiled with their skin on, dried on a kiln, and the whole ground in a steel corn-mill: none of the skin has been separated by dressing.

"By experiments that have been before made on fine dried flour of potatoes, it is known, that it will keep longer than the flour of wheat without spoiling; that it is used as a substitute for sago, and makes good biscuits without admixture. And I have every reason to believe it will mix and make good bread, in a much larger proportion with wheat-flour, than has hitherto been employed of the boiled root, in the common mode of using it.

"Of the expense of preparing the flour from the root in large quantities I am not prepared to speak. The chief labour is washing the potatoes from the mould, which adheres to the eyes, particularly in those sorts, the eyes of which are much depressed. Drying them will be considerably expensive; but I think may be reduced much below what at first it will be estimated at. Grinding will not cost more than corn.

"From what I believe were accurate experiments, I find that one hundred pounds of washed potatoes will produce

produce full twenty-five pounds of flour (such as the sample.) The difference in weight will be very little, whether the potatoes are boiled, or only ground in an apple-mill, and the juice suffered slowly to drain from them before they are dried. It might seem therefore, at first view, that the boiling might be omitted; my trials however have shewn me, that the colour of the flour is much fairer when boiled, and the taste more pleasant; and that the expense of boiling in steam is very little. With the greatest care even some of the starch (the most nutritive part of the root) will separate with the juice; above three pounds of the fine starch (weighed after it was dried) passed off with the water from 100 lbs. of potatoe.

"Other persons will, I trust, ascertain such facts with more accuracy; I myself hope soon to ascertain more satisfactory particulars. In the mean time permit me to make an estimate of the probable produce of an acre of potatoes in quantity, when reduced to the state of flour.

"The average produce of an acre, managed with care, is estimated at about eighty sacks of 240 lbs. each.

"According to my experiments (as before), 100 lbs. of washed potatoes will produce 25 lbs. of dry flour; or each sack 60 lbs.; or one acre, two tons and upwards.

"I am not qualified at present to carry these calculations farther — if quantity alone be the question, I need not.

"Note. The potatoes used in the foregoing trials were the red apple potatoe.

"The steel mill has not ground this flour so fine as I believe a stone mill would have done. Some of these had their skins stripped off after boiling. Should an expeditious method be found of stripping off the skins, it

will

will perhaps be less troublesome than washing so carefully as must otherwise be practised."

After giving a numerical account of the samples of flour of potatoe prepared for exhibition; this gentleman gives also samples of bread and biscuit made from different sorts of potatoe-flour, mixed with different proportions of wheat-flour of different degrees of fineness; but these would be unintelligible in this place, in the absence of such samples.

"The potatoe-flour used in the bread and biscuit is made of the whole of the potatoe, washed, steamed, bruised slightly after steaming, dried on a malt-kiln, and ground in a common corn-mill, no alteration whatever having been made in the set of the stones, from what they were as used for grinding wheat; it may reasonably be supposed however, that a miller, accustomed to grind this article, would make better work and finer flour.

"Nothing was taken from the flour except some large pieces that were not ground, and a little large bran in the proportion of the sample sent herewith.

"The potatoes of which this flour was made, were certainly over-dried; and having lain in an heap after steaming upwards of two days before they were put upon the kiln, some degree of fermentation had begun to take place, but which was thought so little as to have been perfectly corrected by the drying. In the bread, however, it is certainly distinguishable. The baker considers that it is from this cause that the bread is not so light as it otherwise would have been. It rose well in the oven, but fell when the door was opened. He thinks, that when mixed with the flour of dry wheat the potatoe-meal will have exactly the same effect as the mixture of a certain portion of cone-wheat flour, and that it will answer as well in about the same proportion. He has no doubt

but that even with this flour he shall succeed better in the second attempt. With potatoe-meal, well-made, he believes that bread of the best quality may be produced.

"The chief precautions necessary in making potatoe-flour seem to be, to prevent any fermentation taking place in the boiled potatoes previously to their being dried, and to avoid giving them too great a heat in drying. With this view it seems adviseable to construct the apparatus for preparing it, so as that the steaming-tubs and kiln should be heated by the same fire, without loss of time or labour; the potatoes may then be immediately removed from the steam to the kiln; and means should be used to regulate the heat of the kiln, so that it should not much exceed 90°.

"For the common purpose of bread, it seems evident, from the samples, that taking off the rind or skin is by no means necessary; to wash the potatoes carefully before boiling seems, therefore, the only precaution required.

"From experiments as before stated, the produce of dry meal is to the raw potatoe, as 26 or 27 to 100, but let it be estimated at 25, or one-quarter of the whole. The greatest quantity of raw potatoes said to be used as a mixture with wheat-flour in bread is one-third; not much above the same quantity of boiled potatoe has usually been employed. The proportion of flour in boiled potatoe exceeds that in raw potatoe by about one quarter. As a rough ground for calculation, we may therefore call 33 *per cent.* as the proportion of flour in any given quantity of boiled potatoe.

"The proportion therefore which the potatoe-meal makes of the whole mixture in this bread, above that in which one-third raw potatoe has been used, is four times that is, the actual quantity of potatoe-flour in this bread

is as great as if 24 lbs. of raw potatoe had been mixed with 12 lbs. of wheaten-flour; and compared with boiled potatoes, it is as great as if 18 lbs. of potatoe had been mixed with 12 lbs. of wheat flour."

From the foregoing statements, it is not presumed that much farther information is imparted than may have been gathered from some former accounts of bread-making from a mixture of such flours, except as to the mode of preparing the potatoe-flour. Neither is it at present supposed that for common use, when corn is not dear, the potatoe will supersede the use of neat wheat-flour for family bread. But in very dear times, when it may be used in some places to great advantage, the most economical mode of doing it is important; and the process of steaming, kiln-drying, grinding, and dressing, seems excellent. If equal quantities of wheat and potatoe flour are found to make very good bread, and the potatoe to have the effect of cone-flour in the mixture; this may be set down as a sufficient regulation, and a valuable fact.

But what is of great consequence to be known, and fully noticed, is, that the flour of the potatoes so prepared, if barrelled up, and kept in any common dry place, will retain its virtue longer, either on land or at sea, than the other sorts of flour made from grain: in short, from frequent appearances, and well-attested facts, the flour of this vegetable, prepared as aforesaid, seems to possess the singular quality of being almost imperishable. In addition to that quality, the power of preserving potatoes in barrels, after being kiln-dried, either when whole or cut into parts, for the use of the table in long voyages, is very important! and it is found that, after being so preserved, they are capable of being again boiled soft, and served up as a vegetable at table, retaining much of their original flavour, consistence, and other qualities.

Account of the Method adopted for improving Rye Harbour. By the Reverend DANIEL PAPE, *of Penn, near Wolverhampton.*

With an Engraving.

From the TRANSACTIONS of the SOCIETY for the Encouragement of ARTS, MANUFACTURES, and COMMERCE.

The Gold Medal of the Society was presented to the Author for this Communication.

RYE Harbour, once so very safe and convenient for passing vessels up or down the channel, to run to in distress or in precarious weather, had been for many years, and from various causes, in a gradual state of decay, insomuch that in the years (I believe) 1795 and 1796, it was thought necessary to send Captain ———, from the Trinity-House, to make a survey, and report to the Board its then state, and the probability of its improvement or redemption. The survey was made, I believe, with considerable care and attention; and the result was, that the harbour was pronounced lost, or in such an irreparable decayed state, that it was an useless expense to the ships passing, which paid tonnage to it; and therefore this tonnage was taken from Rye, and given to Ramsgate Harbour, leaving however a reserve in the hands of the Commissioners of 6000 *l.*

The consequence of this was an advertisement, inviting any gentleman to come forward with plans for the improvement of the Harbour, and the draining of the upper Levels. On the day appointed for the presentation of such plans, a very sensible letter was laid before the Commissioners by the Rev. Mr. Jackson, of Rye, though impracticable on many accounts,—and also a plan by myself, proposing to make the present cut, and to form a dam

a dam of straw or hay and faggots, as represented on the chart, for the small sum of 500*l.* On reverting to the enormous sums that had been already, from time to time, expended by able engineers to no purpose, it was judged at the moment an impossible attempt; and, after politely voting me their thanks, the Commissioners seemed to decline carrying their plan into execution.—This, however, did not satisfy me; and therefore, confident of success, I undertook to perform what I had proposed, or lose the money, without stipulating for any fee or reward should I succeed. On entering upon this agreement, I set to work, and choosing a Mr. Southerden, an active aud persevering man, as foreman, to assist me, I completed the work in three months, in the very depth of winter, at the expense of only 480*l.* though the works were twice filled up with sea-beach by the tides. But, though this was done to the astonishment and admiration of many, yet there were evidently an envious few mortified and disappointed. The cut and dam being thus finished, it was then thought necessary, on my recommendation, to secure the cut from reverting to its late reduced state, by a pier-head on the East, and jutties on the West side of it; the execution of which was committed to the eminent skill of a Mr. Sutherland, who performed the trust reposed in him, to the universal satisfaction of his employers; and I believe the whole was completed for something less than 3000*l.* in a very masterly and workman-like manner. Of this I think there cannot be a better proof adduced, than that it still stands firm, without the least apparent decay, and maintains its first position without the smallest variation; and no doubt a very trifling annual expense will keep it in its present improved state.

The advantages derived from it are particularly great; for ships of 250 tons burthen, and even vessels of 300 tons,

run

run in with the greatest safety at spring tides; whereas, before, those of 50 tons could not come in, but with the utmost difficulty and danger.

That part of Romney Marsh too, which lies contiguous, and was threatened by every boisterous tide with a total overflow, is now in safety, and the drainage of the levels is rendered complete.

I beg leave now to offer to your attention a short description of the Dam, the form and materials of which may be used with success in similar situations, whether in places adjacent to the sea, or in gentlemen's fish-ponds, or rivers in the country, where weirs may be necessary for the preservation of the banks. The dam was merely formed of hay, straw, and faggots, pinned down to a foundation of sand or silt by short piles. I formed it as in the chart, of the shape of a double-roofed house, first putting down straw, and then over it hazel faggots, from 12 to 14 feet in length, and afterwards pinning down the whole with piles. I next filled the space between the two roofs with gravel or sea-beach, and secured this also with faggots pinned down upon it, over which resistance being precluded from its peculiar form, the influx and reflux of the tides glided so gently, that consequently every probability, not to say possibility, was annihilated of its being ever undermined or blown up.

It was also necessary that this dam should be put down in one tide, and that the mouth of the cut should be opened in the same time; for it was evident to me, that it was impossible ever to cut to sea in any other way. For unless the dam had been ready to turn the water through the cut as soon as opened, and the cut, on the other hand, ready to receive the current the moment the dam began to act, the whole work must have been entirely and unavoidably destroyed by the influx and reflux

of

of the ensuing tide. All this I clearly foresaw; and by procuring a sufficient number of men, nearly three hundred, the business was completely finished, just as the tide touched the foot of the dam; and when it was full sea, the straw of course acted as a receiver and retainer to the silt brought in by the tide; which being repeated by each returning tide, the dam soon became entirely fixed, beyond a possibility of ever being destroyed; and it is now so entirely covered, that if the pier is kept in repair, the dam must ever remain unimpaired by time, and proof against the most violent floods of waters.

A certificate from Mr. Daniel Slade is annexed to the preceding account, stating that he occasionally lived at Rye, is acquainted with Rye-Harbour, and with the Rev. Mr. Pape, whose Memorial respecting the cut and dam at that place he has read, and believes what he has therein related to be strictly true.

REFERENCE to the IMPROVEMENT of RYE-HARBOUR.

(Plate IX. Fig. 3.)

A A, the double roof, filled with straw. B B, hazel faggots, 12 to 14 feet long. C, the space betwixt the roofs, filled with gravel or sea-beach. D, the faggots which covered the gravel so laid. EE, piles of wood driven through the faggots and straw into the earth, at the bottom of the river, the heads of which piles are united by cross pieces of wood. F, the solid bed of the river. G, the river at low water. H, the high-water mark. I, the upper side of the dam, which opposes the current of the river. K, the lower side of the dam, which resists the coming-in of the tide.

Description

Description of an improved Lathe.

Communicated by Mr. G. SMART, *of Ordnance Wharf, Westminster-Bridge.*

With a Plate.

HAVING occasion to manufacture great numbers of picket and tent poles, I have contrived a lathe, by means of which, after they are sawn into an octagonal figure, they are with great dispatch turned into smooth and straight cylinders, all exactly of one diameter. I conceive that this lathe would be useful for other purposes, and have therefore sent you a drawing of it, which I trust the following description will fully explain. (See Plate IX.)

A B C, Fig. 1, represent the puppets of a common lathe. D, is an additional one, fixed by a wedge under the bed, to receive a square sliding bar *d*, which is advanced and withdrawn by a screw *c*, turned by a winch. The contrivance for making the screw draw back the bar *d* is shewn in Fig. 2, where *c* is the screw, turned to a nice point, to work in a hole in the end of the bar *d* when it is advanced; and *a* is a small groove, cut in the screw, to receive the ends of two plates of iron *b b*, which are screwed on to the end of the bar *d*, to form a collar to draw it back. Between the two puppets B and D two slide bars of oak E F are firmly fixed, and are also further steadied by three puppets G G G, fixed by wedges; they are for the tools L and M (which are shewn separately in Figs. 3 and 4,) to slide upon. The mandrill H is driven with a velocity of 1,300 revolutions *per* minute, by a wheel turned by two men. I, is a post, fixed to the bed; on the top of which is a pulley, to keep the two parts of the

the band from rubbing together at the place where they cross each other. The end *f* of the mandrill projects some inches beyond its collar in the puppet B, and has three points in the end *f*, one in the centre, nearly as long again as the other two, near the circumference, to prevent the pole from slipping.

Fig. 3 represents the tool which is first used for roughing off the angles of the work. A B are two boards, screwed on to the block C; the inside edges of which slide against the outsides of the two slide-bars E F, Fig. 1, and the projection E of the block slides between them. Through the middle of this block is a hole D, for the pole while turning to pass through. At one side of the hole, and projecting a little way into it, is a gouge *d*, fixed to the block by two screws at the end *b*, and supported by a piece of wood *e* fixed to the block.

The other tool, Fig. 4, is for turning the pole smooth; it has two boards A B, and a projection E, the same as Fig. 3. The hole through it at D is surrounded by an iron plate *f*, to strengthen it, where the wood is cut away at H, to form an opening for a common plane-iron G, fixed by two screws on the sloping part of the block, in such a position that the iron is nearly a tangent to the circle it is intended to cut, but stands inclined in an angle to the axis of the cylinder, so that one corner of it cuts before the other; and, to assist which, the edge of the iron is whetted a little convex.

When this machine is used, the two tools (Figs. 3 and 4) L, M, Fig. 1, are placed on the slide-bars E F, the finishing tool M being nearest to the puppet D, which they are both slid close up to, the pointed bar *d* being then long enough to project beyond the holes in them; so that the pole, which is before cut to the octagon, (by a cir-

cular saw,) may be fixed in the lathe, by driving one end of it against the points in the end *f* of the mandrill, and screwing the points of the bar *d* against the centre of the other end. The wheel and lathe is then put in motion, and the roughing tool L pushed along the slides E F, gouging the pole as it goes, in the manner of a screw or spiral. When it reaches the other standard B, the smoothing tool M is pushed forwards also, which leaves that part of the pole over which it has passed, perfectly smooth and straight. The lathe is then stopped, the handle of the screw *c* is turned back, and the pole taken away; for which purpose the mandrill *f* projects beyond the holes in the tools L and M, when they are shoved up to the standard B; when this is done, the tools are slid back to the standard D, and another octagon pole is put in, as before.

For turning longer or shorter poles, the two puppets C and D can be moved along the bed, and fixed by their wedges, and the two slide-bars E F are fastened to the puppet D, by a bolt going through them.

By this machine a pole of yellow deal, five feet and a half long, and two inches diameter, can be turned from an octagonal prism to a perfect cylinder in half a minute; and an ash pole of the same size in one minute. Two men to turn the wheel, and one to work the tools, will turn 600 deal poles, as above, in twelve hours.

Fig. 1.

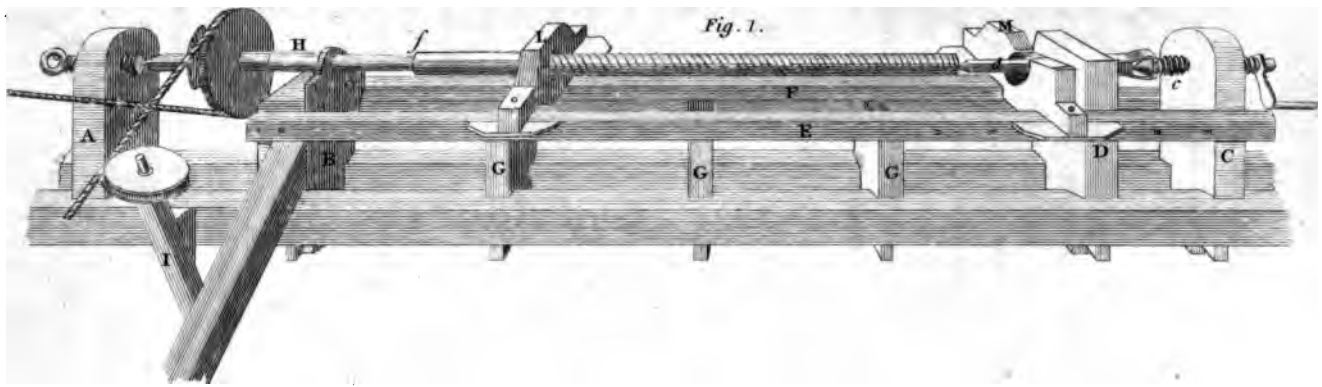


Fig. 2.

Fig. 3.

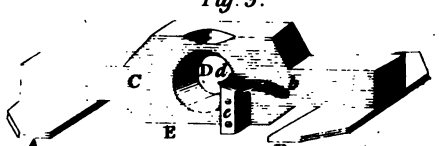
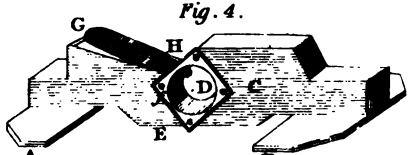


Fig. 4.



Inquiries concerning some Combinations of Mercury.

By M. ZABOADA.

From the JOURNAL DE PHYSIQUE.

WHAT is corrosive sublimate? In what state does the muriatic acid, which is one of its principles, exist in this salt? I think that after the experiments of Mr. Chenevix, all chemists are agreed that it is in that of simple muriatic acid, and that the mercury is oxygenated at its maximum: but as the proportions of the constituent principles of this salt given by Mr. Chenevix differ greatly from those which I have found, I think it incumbent upon me to make them known to chemists.

Analysis of Corrosive Sublimate.

The data which I employed in this analysis are the following: 1, To determine the quantity of muriatic acid existing not only in corrosive sublimate, but also in all the muriatic salts, we must know that this acid has more affinity with the oxyd of silver than with the other salifiable bases, and that 100 grains of silver give 133 grains of muriate of silver, which contain 25 grains of muriatic acid, &c.; 2, that the muriate of tin reduces the solutions of mercury; 3, that sulphurated hydrogene also reduces them; 4, that pure potash decomposes them completely; that we may obtain corrosive sublimate not only by means of oxygenated muriatic acid and mercury, but also by muriatic acid and red oxyd of mercury. These data being established, I shall proceed to the detail of what I have observed in my experiments.

Corrosive Sublimate and Nitrate of Silver.

I dissolved 100 grains of corrosive sublimate in 32 ounces of hot water; I added successive quantities of ni-

trate of silver till the solution was no long turbid. After having washed the muriate of silver in distilled water, in order to dissolve all the nitrate of silver and mercury that might remain in it, I dried it in a small porcellain cup of a known weight, and in order to assure myself that it was perfectly dried, I also heated it in a small retort, where it was fused, but without losing any of its weight, which was 102 grains: 100 grains of corrosive sublimate contain, according to the common opinion, 18 grains of muriatic acid; but as 100 grains of silver give only 130 grains of muriate, as I have observed above, 100 grains of corrosive sublimate contain in reality only 161 grains of muriatic acid, supposing that there are always 9,5 grains of oxygene in the 130 grains of muriate of silver.

Corrosive Sublimate and Muriate of Tin at the minimum of Oxygene.

I put into two small cups 144 grains of corrosive sublimate (72 into each); and I added nearly two ounces of solution of tin. This mixture which at first was white, gradually blackened. Nevertheless the tin, which was at the minimum, could not at once seize the oxygene of the corrosive sublimate; it therefore reduced it first to the state of sweet mercury, and at last to that of pure mercury. This was collected in the form of a black powder, which with the aid of hot water united into several small globules. These, after being dried in the sun, weighed 51 grains. Accordingly 100 grains of corrosive sublimate should contain 71 grains of mercury.

Corrosive Sublimate and Sulphurated Hydrogene.

Sulphurated hydrogene decomposes corrosive sublimate in proportion to the greater or less quantity of it that we employ, so that we may obtain, according to this quantity, either sulphurated sweet mercury or mercurial ethiops.

ethiops. I have repeated this experiment, the discovery of which belongs to M. Proust, wishing to confirm that in 100 grains of corrosive sublimate there are 71 of mercury, supposing that 100 grains of cinnabar are composed of 85 of mercury and 15 of sulphur; but before proceeding to this proof, I wished to ascertain whether the proportions above-mentioned were true, but I found a result very different from that which I sought.

I dissolved 100 grains of mercury in 200 grains of boiling nitric acid; as soon as the nitrous gas had disappeared, I let the solution cool; I then caused to pass through this a sulphurated hydrogene gas, and I observed that black flakes suspended in the liquor successively grew white. At last, after a certain time, the white precipitate which they formed beginning to grow black, I stopped the application of the sulphurated hydrogene, and stirred the precipitate, which soon recovered its white colour. (In order to obtain this result, it is necessary that a small quantity of nitrate of mercury remain in the solution.) What is this result? The nitrate of mercury at the maximum of oxygene and of acid, and the sulphurated hydrogene concur in its formation: will the same thing happen with this salt as the corrosive sublimate experiences in the like case? The results are different, but their reduction follows the same law.

1. When we heat it gently in close vessels, we obtain a mixture of nitrous and sulphurous gas: on examining the residuum, it is found to have lost hardly any thing of its whiteness. If we expose it to a greater degree of heat, we still find the same gaseous products; it then passes into the black, and is afterwards completely reduced, but it does not yield any cinnabar at all.

2. The nitric acid at 15° of heat has no action upon it; the same acid at 40° and boiling dissolves it very little, since

since after filtration potash does not demonstrate its presence in it; but sulphurated hydrogene shews that a very small quantity of it has been dissolved by the acid.

3. Boiling concentrated sulphuric acid decomposes it slowly, and the results of this decomposition are, 1, nitric acid, which is disengaged; 2, sulphur which is precipitated; 3, sulphate of mercury at the maximum of oxygene and of acid.

4. Pure potash decomposes it, and this mixture is seen to assume at first a light yellow colour, which suddenly becomes black; by diluting it with water, filtrating and evaporating it, we obtain crystallized nitrate of potash. The black precipitate which remains upon the filtre, being well washed and dried, did not dissolve in boiling nitric acid; oxygenated muriatic acid dissolved it with precipitation of sulphur; distilled, it gave cinnabar and mercury. I therefore assert that the white precipitate which I obtained by means of sulphurated hydrogene and nitrate of mercury at the maximum of oxygene and of acid, is a true combination of nitrate of mercury at the minimum of oxygene, of acid, and of sulphur.

To admit this consequence we need only to reflect on what takes place in the experiment, of which I have just given an account. In the first we see that the salt is transformed into nitrous and sulphureous gas and mercury, which proves that it contains sulphur and nitric acid, which in such a temperature obey only the law of attraction established by nature. We see in the second that the nitric acid does not dissolve it, which proves that the sulphur is combined with it; the contrary happens when the sulphur is simply mixed with the nitrate of mercury at the minimum of oxygene and of acid. We see from the third that the sulphur is combined with it, for the nitrate is decomposed by cold sulphuric acid when it is merely

mixed

mixed with the sulphur. From the fourth we conceive that potash decomposes it by reason of four affinities concurring together; 1, that of the potash for the sulphur, and hence proceeds the light yellow colour which is observed in beginning the mixture; 2, that of the potash, the base of the sulphuret formed by the nitric acid; 3, that of the hydrogene to combine with the oxygene of the oxyd of mercury; 4, that of the mercury for the sulphur.

The nitrate of mercury at the maximum of oxygene and of acid, is reduced not only by sulphurated hydrogene, but also by muriate of tin at the minimum of oxygene. To the solution of this nitrate I gradually added that of muriate of tin at the minimum of oxygene till I obtained a white precipitate. This, after having been well washed and dried, gave by distillation sweet mercury, and we find in the retort a residuum which is oxyd of tin at the maximum of oxygene. If therefore the oxyd of tin takes from the oxyd of mercury at the maximum, the quantity of oxygene which forms the distinction between the oxyd of mercury at the maximum and the oxyd of mercury at the minimum, it gives to the latter the quantity of muriatic acid which it requires for its conversion into sweet mercury. To obtain this result, it is necessary that there remain in the solution a small quantity of undecomposed nitrate of mercury, otherwise the result is mercury and muriate of tin at the maximum of oxygene. The muriate of tin at the minimum of oxygene also decomposes the nitric solution of mercury at the minimum of oxygene and the maximum of acid. This is precipitated at once in the state of sweet mercury, which is reduced only by adding a fresh quantity of solution of tin. Thus the oxyd of mercury at the minimum carries away the muriatic acid from the oxyd of tin, but it

it yields its oxygene to it only when it is dissolved in the muriatic acid.

I now return to my problem. I dissolved in 3 ounces of cold distilled water, 100 grains of corrosive sublimate; I precipitated it by pure potash, and I obtained from 79 to 80 grains of a yellow oxyd. If we make this precipitation at the temperature of 8°, we obtain a brick-coloured precipitate; if we let the solution cool a little, the precipitate is of a blackish red. What may be the cause of these different colours? do they belong to the muriatic acid, to the potash, to the water, or to the different proportions of oxygene? To neither of them. If we heat 100 grains of these precipitates, we obtain from 89,5 to 90 grains of mercury, little or no water, no residuum and no muriatic acid, since the nitric acid dissolves them, and this solution does not render that of silver turbid. The yellow oxyd heated slowly in a small retort becomes red, but it loses nothing of its weight; if we throw it upon a piece of paper, in a moment it recovers its yellow colour. The brick-red oxyd does not lose its colour when heated. Nothing can therefore be inferred respecting the state of the metallic oxyds from their colour.

I dissolved, in a small retort of known weight, 100 grains of mercury in 200 of boiling nitric acid: as soon as the nitrous gas had disappeared, I let the solution cool, and added to it two drams of muriatic acid; immediately the solution became filled with crystals of corrosive sublimate; I then evaporated it by a gentle heat, till the muriate of mercury that had been formed was dried; I now augmented the heat, and after the salt had been fused it sublimed. By this means I obtained from 139 to 141 grains of corrosive sublimate in three times that I repeated this experiment. 100 grains of Dutch red oxyd, distilled

led, yield from 89,5 to 90 grains of mercury, and 100 grains of this same oxyd after having been dissolved by the muriatic acid, and after having been dried and sublimed, give from 125 to 127 of corrosive sublimate.

According to these results, corrosive sublimate is composed of nearly 19 or 20 of muriatic acid, from 8 to 8,5 of oxygene, and from 71 to 72 of oxyd of mercury at 10 of oxygene, and not at 15 which Mr. Chenevix's analysis gives. I shall hereafter term the corrosive sublimate, soluble muriate of mercury, or muriate of mercury at the maximum of oxygene, in order not to confound it with the sweet mercury, which is an insoluble muriate, and at the minimum of oxygene and of acid, as I shall immediately shew.

I accordingly proposed to myself the following question. What is the difference between the sweet mercury which is obtained by subliming a mixture of soluble muriate of mercury and mercury, and the white precipitate which is obtained by precipitating a nitric solution at the minimum of oxygene, and at the maximum of acid by the muriate of soda? To resolve this question, these two muriates must be analysed, and the results of this analysis compared.

1. I dissolved 100 grains of sweet mercury in 200 of nitric acid; I mixed the soluble muriate of mercury and the nitrate of mercury that were formed, with distilled water, and added nitrate of silver: the muriate of silver which I obtained, when washed and dried, weighed 60 grains, which represent 10,6 of muriatic acid *. From 100

* I think, according to the muriate of silver which I obtained in the analysis of the soluble muriate of mercury, and in this, and according to the proportions which I found in the two muriates, that I may give for the proportions in 180 grains of muriate of silver, 7 of

100 grains of white precipitated muriate I obtained the same result; nevertheless I must mention that its solution and the superoxydation of its base was most speedily effected, which merely proves that it was in a state of more complete division.

2. 100 Grains of sweet muriate disoxydated by the muriate of tin, gave me from 84,5 to 85 grains of mercury. I obtained an equal result from the same quantity of precipitated white muriate: but 85+10,6=95,6; and 100—95,6=4,4 oxygene. There is, therefore, no difference between these two muriates. In the 100 grains of the oxyd, the base of the two muriates, there enter 5 of oxygene. This quantity differs from that found by Mr. Chenevix by 5,7 grains (Van Mons's Journal, N° 8, p. 127).

3. 100 Grains of precipitated white muriate, well dried, give 100 of sublimed sweet mercury.

4. 100 Grains of these muriates having been dissolved in a mixture of nitric and muriatic acid, by evaporating and afterwards subliming, from 117 to 119 of soluble muriate of mercury are obtained. Thus 100 grains of these insoluble muriates, to become soluble, combine with 18 grains of oxygene and muriatic acid in this manner:

Mercury belonging to 100 grains of insoluble muriate	85,0
Oxygene belonging to it	4,4
Oxygene augmented	5,0
Muriatic acid of 100 grains of insoluble muriate	10,6
Muriatic acid augmented	13,0
	118,0

oxygene, and 23 of muriatic acid. I could not obtain these results by precipitating the solution of 100 grains of silver by lime-water or by pure potash.

After

After having solved this problem, I proposed to myself the following: Is it well demonstrated that the precipitates of the nitrates and muriates of mercury retain a portion of acid? (Annales de Chimie, tom. 38, page 126.)

The experiments of Bayen have placed it out of doubt, that the precipitates of mercurial nitrates and muriates retain a more or less considerable portion of acid, so that if we expose to a sufficient heat the precipitates of the mercurial nitrate, nitrous acid is disengaged, and if we perform the same operation upon the precipitates of the mercurial muriate, a more or less considerable quantity of an insoluble mercurial muriate is sublimed—(Bayen, Tom. I. p. 223, Exp. XII.) Half an ounce of this precipitate having been exposed to calcination in a small high and narrow glass vessel, nitrous vapours were disengaged from it—(p. 23). Fixed alkali does not completely decompose corrosive sublimate, and the precipitate gives sweet mercury—(p. 238). An ounce of this precipitate being put into a retort and exposed to the fire, there were sublimed 4 drams 48 grains of sweet mercury. How is the disoxydation of the soluble muriate of mercury effected by caustic potash, and by the carbonate of potash? This is what I cannot comprehend, notwithstanding the explanation given by Bayen (p. 240). In order to give a sufficient reason for this singularity, we must, I think, refer the cause of it to the solubility of corrosive sublimate, and the insolubility of sweet mercury, two of the distinguishing characters of these chemical preparations. Corrosive sublimate contains as much marine acid as the mercury is capable of combining with, which renders it soluble in water; sweet sublimate on the contrary contains the least possible quantity of it, and therefore it becomes insoluble. I accordingly separated some of Bayen's experiments, the result of which

not only gave me the solution of this problem, but I likewise found means of obtaining a nitric solution of mercury, completely at the minimum of oxygene.

1. I dissolved 100 grains of mercury in 100 grains of cold nitric acid at 33°. After this solution was crystallized, I separated from the small needles of nitrate of mercury which were formed in it, upwards of 12 grains of mercury, and I observed that this dissolved in it (A), but without the formation of nitrous gas. The other half of the residuous liquor having been decomposed by the muriate of soda, and after having separated the insoluble muriate of mercury, I found in the filtrated liquor soluble muriate of mercury, as potash precipitated it in yellow oxyd, and sulphurated hydrogene in insoluble sulphurated muriate, which soon passed into the state of ethiops, on adding a fresh quantity of sulphurated hydrogene. The solution (A) precipitated by muriate of soda, gives hardly any soluble muriate, for the filtrated liquor was not rendered at all turbid by potash, and very little by sulphurated hydrogene.

2. I dissolved 100 grains of mercury in the same quantity of cold nitric acid at 30°, and I observed that the nitrous gas exhaled from it was less than in the first experiment. In this I obtained somewhat larger crystals (B), and the rest of the mercury weighed three grains. The residual solution, after having been decomposed by the muriate of soda, and filtrated, was little disturbed by sulphurated hydrogene.

3. Four ounces of mercury were dissolved by the same quantity of cold nitric acid at 26. The nitrous gas that formed was in very small quantity, and dissolved entirely in it, giving it a blueish colour. This solution did not immediately crystallize, but after some days I observed upon a slight agitation a multitude of small crystals: I separated

separated the liquor from them, and after having precipitated it by the muriate of soda, and filtrated it, it still contained a small quantity of soluble muriate of mercury.

It is evident from these experiments, that it is difficult to obtain a nitric solution of mercury completely at its minimum of oxygene; but if we dissolve the crystals (B) in distilled water, adding as much nitric acid as is required for dissolving the nitrate of mercury at the minimum of oxygene and of acid which is precipitated, we obtain a solution of mercury at the minimum of oxygene and at the maximum of acid. This solution, precipitated by the muriate of soda, is completely converted into insoluble muriate of mercury, since, after it has been filtrated, sulphurated hydrogene does not render it at all turbid.

In these experiments, I could find only two muriates, whereas M. Berthollet found three; nor could I find the nitric acid which that chemist observed, nor the intermediate degrees of oxydation.

"When we precipitate," he says (Annales de Chimie, Tom. XXXVIII. p. 119) "the nitrate of mercury with muriate of soda, we obtain mercurial muriates which differ according to the oxydation of the mercury; with the nitrate little oxydated we have a white precipitate which retains a portion of the nitric acid, which cannot dissolve in muriatic acid, and which does not dissolve in concentrated nitric acid without the evolution of a *large quantity* of nitrous gas; with the solution prepared hot we obtain a *slightly yellow* precipitate, which is not soluble in the muriatic acid, but which dissolves easily in nitric acid, evolving *little* nitrous gas; the liquid that covered the precipitate gives by evaporation a small quantity of corrosive muriate of mercury."

But

But in the fluid nitrate of mercury, there are no determined proportions between the most oxygenated and the least oxygenated mercury; it appears that all the intermediate proportions *may* exist, as I have observed with respect to the sulphates. (Ibid. p. 128). The sulphates follow the same law as the nitrates of muriates; their base is always combined either with 5 or with 10 *per cent.* of oxyd. Bergman and Scheele were already well acquainted, that mercury dissolved in nitric acid may exist in it in two different states. Scheele says, that when cold nitric acid can dissolve no more mercury, we should add to it a fresh quantity of mercury and boil the mixture. Mr. Chenevix confesses that he does not see the reason of this (Van Mons, N°. 8, p. 128). Scheele did not know at that time, that mercury in order to combine with the acids must previously combine with one or two constant proportions of oxygene. Hence we see why he could not explain what happens in this solution. The mercury which is added to it is oxydated by taking from the oxyd at the maximum which exists in it half of its oxygene; that is to say, by reducing to five the oxyd which is oxygenated at 10 *per cent.* This is the reason why the mercury disappears, and we see no nitrous gas till the oxyd of mercury at the maximum returns to its minimum. This law is invariable with all the mercurial salts; not only the muriate becomes again insoluble, that is to say, at its minimum of oxygene and of acid, by a new quantity of mercury, but also the sulphate of mercury at the maximum of acid, and at the minimum, and at the maximum of oxygene. I mean to speak of that white mass which we obtain by distilling a mixture of one part of mercury, and one and a half of concentrated sulphuric acid. If we mix this mass with one-third of its weight of mercury, and heat it, we reduce it almost entirely

tirely to its minimum of oxygene and of acid. Thus we cannot obtain this combination in a direct manner, but we effect it by precipitating the solution of the mercurial nitrate (B) by another of sulphate of soda. On the contrary, we obtain the solutions at the maximum of oxygene and of acid in a direct manner; for this purpose we must keep over the fire the nitric and sulphuric solutions of mercury in proper proportions, till the nitrous or sulphurous gas ceases to appear.

Now that I have well ascertained the means of obtaining nitric solutions at the minimum and at the maximum of oxygene, I return to the solution of the second problem.

1. I precipitated a nitric solution of mercury at the minimum of oxygene, and at the maximum of acid, by pure potash, and I obtained a black precipitate, which, after having been washed and dried, was heated in a retort to the quantity of 50 grains, where it was reduced without disengagement of nitrous gas, and the mercury weighed from 47 to 47,5 grains. A hundred grains of black oxyd are therefore composed of 95 of mercury and 5 of oxygene.

2. I precipitated the same solution by the carbonate of potash, and I observed that the precipitate which was formed was white, but that it suddenly assumed a whitish-yellow colour: I suspected at first that it passed in part into the maximum of its oxydation, which I confirmed by means of muriatic acid, which dissolved a little of it. This precipitate lost by the concentrated sulphuric acid 0,10 of carbonic acid: heated in a small retort it gave no nitrous gas, hardly any water, and the mercury which resulted from it weighed 83 grains. I believe that in this precipitate the mercury is not saturated with carbonic acid: I could not repeat this experiment in order to see whether

whether I could obtain a saturated carbonate completely at the minimum of oxygene.

3. The nitric solution of mercury, at the maximum of oxygene and of acid, was decomposed completely by pure potash, and the precipitate which I obtained was of a yellowish colour. This being washed, dried, and calcined, was reduced without any nitrous gas, and the mercury weighed 90 grains.

4. The same solution, precipitated by the carbonate of potash, gave a precipitate of a brown-red colour. The liquors of these precipitations did not contain any oxyd of mercury, as sulphurated hydrogene did not demonstrate the presence of any in them. This brown-red precipitate lost 0,12 of carbonic acid by the sulphuric acid: distilled, it was reduced without any traces of nitrous gas, and hardly any of water, and the mercury weighed 80 grains. (I believe also, that in this precipitate oxyd of mercury is not saturated with carbonic acid.) Thus the nitric solutions of mercury are completely decomposed by potash, and by the carbonate of potash, and their precipitates do not contain the least quantity of nitric acid.

I have already shewn that pure potash completely decomposes the soluble muriate of mercury, and that the precipitate which is formed contains no muriatic acid. Let us now see what effect is produced upon this salt by the carbonate of potash.

1. I poured into a solution of muriate of mercury, in cold water, another solution of carbonate of potash, in sufficient quantity, and till the liquor no longer became turbid. I thus obtained a purple precipitate. The liquor (C) gave with sulphurated hydrogene an abundant precipitate of mercurial ethiops. This fact, therefore, demonstrated by Messrs. Bayen and Berthollet, is correct;

rect; for they say that the carbonate of potash does not completely decompose the corrosive sublimate, and also that this precipitate gives sweet mercury; but this is not found formed in it; it is formed, as I have discovered, by heating it.

1. Cold nitric acid at 10° completely dissolves the purple precipitate, and this solution precipitates that of silver. It therefore contains muriatic acid.

2. I heated in a small retort 20 grains of this precipitate, and I obtained from 10 to 11 grains of insoluble muriate of mercury, which contained a small quantity of soluble muriate.

3. I exposed to a gentle heat the solution (C), and I observed that there was disengaged from it a considerable quantity of carbonic acid, and that, in proportion as this acid was volatilized, there was precipitated a powder which, looked at through the liquor, appeared black. This precipitate presented itself crystallized in laminæ, its colour was a dull violet; it dissolved very well in water, as also in cold nitric acid at 10° with disengagement of carbonic acid, and this solution precipitated that of silver: heated in a small retort in the quantity of 20 grains, it gave me from 10 to 11 of insoluble muriate, and a small quantity of soluble. Why does the soluble muriate become insoluble? What is it that takes from it half of its oxygene and of its acid? It seems to me that the degree of heat at which the soluble muriate sublimes, is sufficient for disoxydating a small quantity of the oxyd of mercury, and that then this, subliming in the state of mercury with the soluble muriate, transforms it into insoluble.

Thus I have exhibited results different from those of Messrs. Bayon and Berthollet, and which shew that, in order to become well acquainted with the metallic salts,

as well as with the precipitates that are obtained from them, we must previously know the state of oxydation of the metals in the acids, by determining their degrees numerically, after having demonstrated them by chemical means; and if Messrs. Bayen and Berthollet have obtained results different from mine, it is because the former did not know the nitric and muriatic solutions of mercury, and decomposed them by impure alkalies; whilst M. Berthollet, on the contrary, well knew the purity of his alkalies, but has always operated on nitric and sulphuric solutions of mercury which contained this metal in two states of oxydation.

Of the Influence of Manganese in the Production of Iron on a large Scale. By M. STUENKEL, *Junior. Translated from the German by M.* DAUBUISSON.

(Concluded from Page 124.)

STEEL is a substance intermediate between cast iron and malleable iron; for steel kept for a considerable time in the refining fire is converted into iron. In order to convince ourselves of this, we need only melt over again a bar of steel, ready to be forged, instead of putting it under the hammer, and we shall then obtain a new bar, which will no longer be steel, but perfectly malleable iron. The whole process of the fabrication of steel consists in keeping the cast metal for a sufficient length of time (but no longer) in the fire, for it to assume the first degree of malleability, or, in other words, for it to become steel; and till the mass, being almost fluid, has a sufficient consistence to be put under the hammer, and to admit of being hammered *a little*; in a word, the bar

must

must still be in some measure cast metal, but it must not be such equally throughout.

It is possible to conceive that the *grey* cast metal of which we have been speaking may be brought to this state; but its execution is not attainable in practice. The cast metal, after having been cast in a mass is too fluid: the part which is acted upon directly by the wind forms immediately solid steel, whilst the rest is still cast iron; and when we endeavour to bring the remainder to the same state, the same causes which produced the first pieces of steel continue at least in part their action upon these, which then become malleable iron; and if the operation does not continue long enough for the whole to be in this state, we obtain a mass which is partly iron and partly steel *.

Such are the principal differences which distinguish the *grey* cast metal from the *white*. I shall now proceed to treat of the cast iron which is obtained by the treatment of the manganesiferous ores.

Of the Cast Iron from Manganesiferous Ores, or of the White Cast Iron.

The white cast iron may also be divided into the smooth and harsh: the first is obtained by putting an excess of charcoal into the charge; the second by charging the furnace with an excess of mineral. The smooth white cast metal is very fluid as it comes out of the furnaces, more so than any of the varieties of the grey metal; but it becomes fixed more quickly than they do, and it is thus more fluid, but less fusible. As it cools it assumes an even surface, under which some small bulbous

* I do not know that there exists a single manufactory of steel in places where no use is made of manganesiferous ores.

cavities are discerned. It is very brittle: when rolled into thin plates it cracks of itself; and even when it is in large masses it is easily broken with the hammer. On account of this brittleness it is unfit to be cast into moulds: the rapidity with which it becomes fixed does not even allow it time to fill large moulds. The appearance of its fracture is nearly the same as that of the harsh cast iron of the grey species.

I must here repeat, what I have elsewhere observed, that in places where the manganesiferous ores are mixed with such as contain no manganese, and a considerable excess of charcoal is put into the charges, a cast metal is obtained, the fracture of which considerably resembles that of the smooth or intermediate cast metal of the grey species *; it does not become fixed so easily as when only manganesiferous ores have been melted; it is not so brittle, and may even be employed for casting into moulds. Accordingly, at Gittelde, in the Hartz, where the minerals consist principally of spathic ore and brown hæmatites, cast iron goods were formerly fabricated, but then a surcharge of charcoal was employed.

The harsh cast iron obtained from the manganesiferous ores appears sufficiently fluid as it comes out of the furnace, but it is very heavy, and becomes fixed within a small distance from the orifice; if a little too much ore has been put into the charge, it becomes fixed even in the crucible of the furnace. Its surface and interior are filled with small bulbous cavities. Its fracture is very white, radiated, frequently laminated in large scales;

* If it resembles it in appearance and some of its properties, it differs greatly from it in its manner of comporting itself in the refining-furnace; for it refines with much greater difficulty than any variety of the grey cast iron.

(its

(its colour is like that of antimony, its texture like that of bismuth.) It is harder and more brittle than any other variety of cast iron, and it may be employed even for making wire-drawing irons.

In this species we cannot so easily distinguish an intermediate cast iron as in the preceding species, where the intermediate metal is more fluid than either the smooth or the harsh: here, on the contrary, the cast metal is more fluid in proportion as it is less harsh, and we never obtain a smooth and dense iron.

The cast irons of the manganesiferous ores * contain but very rarely carburet of iron.

In the refining of these cast metals, the harsh kind requires less time than the smooth; it sooner acquires malleability; but even this variety does not refine so easily as the three varieties of the preceding species; and this is what constitutes the principal difference between the two species.

The cast iron of the manganesiferous ores is so difficult to be refined, that not more than from 30 to 36 quintals of iron in bars can be obtained from it in a week, whilst the grey metals yield from 50 to 56.

When we refine the grey cast iron, the part which detaches itself from the pig iron is not entirely fluid, and by this operation it already acquires a certain degree of infusibility and malleability, a small one indeed compared to iron; but after this metal has been wrought, and exposed several times under the bellows, it acquires these properties in a high degree.

* In melting the manganesiferous ores, care must be taken not to employ an excess of mineral in the charges; for in that case the crucible would become obstructed, and the cast iron attach itself to it; an inconvenience which is less to be apprehended in melting the other

The white (manganesiferous) cast iron requires much more time and labour to refine it. As it melts it sinks to the bottom of the fire-place, where it remains fluid, so that it cannot afterwards be moulded into mass in order to be replaced upon the charcoal, and exposed to the action of the wind, as is done in working the grey cast iron. If by cooling the bottom of the fire-place * we cause the melted mass to become fixed, and replace it, either whole or in fragments, under the blast, it melts again much more quickly, and remains more fluid than the grey; so that the operation must be repeated several times. In order to expose it more to the action of the wind, the bottom of the fire-place is sunk less below the pipe, and less slope is given to the latter. In this manner the metal is more in contact with the wind, and is exposed to a less intense heat; but notwithstanding these precautions, the refining requires much more time than that of the grey cast metal.

In some places, where the white cast iron is refined, the fire-place is constructed in a quite different manner: it is made narrow and deep, and a greater slope is given to the tube. This kind of operation is termed *cold refining*, because, after the pig-iron has been melted and the matter is in the fire-place, it is let cool; the blast is stopped, and the charcoal pushed aside: at the end of half an hour, when it is fixed, it is again placed upon the charcoal, and the blast admitted: as this metal must then still remain exposed to the action of the wind till it has acquired the heat requisite for its fusion, and as it is fused but very slowly, it cannot fail to refine well.

* For cooling the bottom, a cavity is formed beneath it, into which any quantity of water that may be thought proper is conveyed when required.

No

No time is gained by this method, for a considerable time is required for cooling. Neither is it attended with any saving of charcoal, for though none is consumed during the cooling, a considerable quantity is required for heating and fusing again the metallic mass.

This method of refining is by no means applicable to the grey cast iron: after the first fusion it is semi-malleable; if we should let it cool, and afterwards heat it again till it melted, we should obtain but little iron, and nearly the whole would be converted into scoriæ; for the same causes which, in refining, converted the cast metal into malleable iron, would convert this into scoriæ, if they acted continually upon it.

A property which particularly distinguishes the white cast metal from the grey, is that the first may be employed for making good steel. Experience has shewn, that the grey does not possess this property, or at least steel cannot be obtained from it with advantage. During its conversion into iron it passes indeed through the state of steel, but this transition is much more rapid than with the white cast metal; this would be no detriment if the operation could be performed in such a manner that all parts of the metal arrived at this state at the same time; in that case we might seize the moment for drawing the metal out of the fire, when we should obtain a homogeneous steel. But the fact is otherwise: the causes which produce the transmutation, namely, the blast of the bellows, and the heat thereby produced, act unequally upon different parts of the metallic mass; so that some pass already from the state of steel into that of iron, whilst the others are not yet steel.

The case is quite different with the white cast iron, since it is refined with greater difficulty; that is to say, it passes more slowly into the state of steel, and afterwards

wards into that of iron; so that it is more easy, by a proper manipulation, to reduce it into steel equally in all its parts, and there is less danger of missing the moment, if I may so express myself, when the whole is steel. This certainly is the cause why the cast metal obtained from the manganesiferous ore is the best adapted for the fabrication of steel. But why is this cast metal not refined as easily as the grey? This is a question which cannot be answered with the same certainty.

The new experiments of M. Lampadius prove that the difference between cast and malleable iron consists in this, that the former substance is iron mixed with carbon and a little oxygene, whilst the other is pure iron; and that, in the refining operation, this carbon is carried off by the oxygene of the wind from the bellows, the iron oxydated, and the foreign matters are then reduced into scoriæ. If steel be iron which contains only a small quantity of carbon, and which is deprived of oxygene and of foreign matters, then the whole fabrication of steel has for its object to separate from the cast iron all the foreign matters which it contains, and to concentrate in it at least a part of its carbon.

All east iron can be converted into malleable iron, but only the white can be converted into steel: if the grey cannot, this proceeds probably from the circumstance, that its carbon burns at the same time with the other substances which it contains; and the immediate cause which prevents this from taking place with the white, is undoubtedly the manganese which it contains. We know that this metal has a great affinity for oxygene, which, under certain circumstances, may be greater than that of carbon for the same substance; this affinity seems particularly to be more considerable when the carbon is already combined with iron. Thus, during the refining of the white cast iron, the oxygene combines with the manganese, the carbon remains at least in part in the iron, and the oxydated manganese passes into the scoriæ with the heterogeneous matters. With the grey

cast

cast iron, on the contrary, the oxygene of the wind combines with the carbon, and takes it from the iron.

If the combination between these substances take place in the manner that has been described, it may possibly be the case that there remains in the white cast iron a certain quantity of carbon after the other matters are separated from it; and the moment when this cast iron is converted into steel is precisely that when the carbon is united in a certain proportion with the iron. If we still leave the matter exposed to the fire, after this combination has taken place, the oxygene continuing to act upon the carbon carries it off, and the steel becomes malleable iron.

What has just been said, explains equally why the cast iron obtained from the manganesiferous ores is more difficult to be refined.

I shall also remark, that though in the fabrication of steel the manganese mixed with the white cast iron is oxydated and scorified in the manner above indicated; it is, nevertheless, probable that a part of it, though a very small one, still remains in the steel, and that this is the reason why the cast steel endures the fire better, does not become soft in it, nor change so easily into iron as the steel of cementation.

Observations on the Carbonate of Potash.

By M. STEINACHER.

From the ANNALES DE CHIMIE.

CHEMISTS know that the carbonate of potash well saturated, so as to effloresce, can only be formed by causing to pass through a cold alkaline solution a quantity of carbonic acid gas, sufficient to determine a spontaneous crystallization. For, on stopping the disengagement of the gas at the moment when the earth of the

alkali appears to be deposited, and evaporating the liquid with the aid of a gentle heat, lamellated crystals are obtained, which soon deliquesce.

Alkaline ley, warmed by means of tan, according to the process of M. Curaudeau, does not succeed any better in furnishing a well-saturated alkali by evaporation. I have found that its crystals become moist; and the author himself acknowledges this slight deliquescence. It is also generally admitted, that the apparatus contrived by M. Welter, and described in the twenty-seventh volume of the Annales de Chimie, is much too complicated, and that of the late M. Pelletier has been adopted in almost all laboratories, with some slight alterations in the disposition of the first bottle, to which is fixed a tube, with a double or triple perpendicular curvature, for the introduction of the acid, or a long pipe of glass, terminating in the form of a funnel. Chalk mixed with water to the consistence of thin soup, is gradually poured into this pipe, which is accordingly stopped with a glass rod, and the gas is forced to pass through the bottles which contain the alkaline ley. This method is, in my opinion, attended with very great inconvenience; for when a tube with many perpendicular bends is used, it must be charged with a column of fluid sufficient to counterbalance the pressure of the carbonic acid gas, and consequently an elevation must be given it which exposes it to be easily broken; and when a long pipe of glass is employed, it often happens that the chalk is exploded into the air at the moment when the piston is opened.

Another method appears to me to be more simple and commodious. It will be found to have some resemblance to that of M. Brugnatelli; but the Italian chemist has not published the details of his manipulation, without which it would be vain to hope to succeed.

A kilo-

A kilogramme of powdered chalk is put into a glass bottle with two or three tubulatures, capable of containing twelve kilogrammes of water. On this is to be poured, without stirring, a litre of a mixture of one kilogramme and a half of sulphuric acid with nine kilogrammes of spring water. The gas is expelled, and a crust of sulphate is formed at the surface of the calcareous carbonate. At the end of two hours all the rest of the acid water is to be added at once, and the bottle quickly stopped.

Bubbles of air are rapidly disengaged, but their disengagement gradually abates, and continues in moderation for twenty-four hours; the mixture is then stirred with an iron rod, and the gas still continues to be developed for twenty-four hours more with little interruption. I discovered the cause of this prolonged effervescence in the resistance opposed to the action of the sulphuric acid on the chalk by the density which the combination successively acquires, which density the tendency of the sulphuric acid to augment the solubility of the sulphate of lime confines within due limits.

As, with the exception of the first bottle, my apparatus is the same with that which M. Pelletier has described in the fifteenth volume of the Annales, I shall not speak of it, but only offer the following remarks:

1. Pelletier was wrong in neglecting to use an intermediate bottle half full of water; this means is very necessary, in order to separate the sulphuric acid which the gas always brings over.

2. The tubes of an inch diameter being too difficult to be bent, may be replaced by others of seven or eight lines aperture, which will answer the purpose equally well.

3. The alkaline solution made in the proportion of 2 lbs. of alkali to 3 lbs. of water, crystallizes too quickly, and before the complete precipitation of the silex.

4. The proportions of alkali and water most favourable to a regular crystallization, at the temperature of from five to ten degrees of Reaumur, are one part of distilled water to half a part of purified potash.

If the results of my experiments shall improve the preparation of the carbonate of potash, if the disposition of my apparatus shall prevent the necessity of continually watching its direction, by procuring, without inconvenience, that gentle and continued pressure of the carbonic acid gas, of which Pelletier perceived the efficacy for the saturation of the alkali, I think I may hope, that the true friends of chemistry, I mean those who practise it, will consider my observations with indulgence.

Experiments on Gum Arabic and Gum Adracanth.

By M. VAUQUELIN.

From the ANNALES DE CHIMIE.

1. TEN grammes of red gum adracanth yielded on combustion 3½ decigrammes of white ashes.

These ashes dissolve with effervescence in muriatic acid, and exhale a smell of sulphurated hydrogene.

Their solution gives with ammoniac a precipitate which is phosphate of lime and oxyd of iron. The oxalate of ammoniac precipitates much lime from it.

Thus red gum adracanth contains to the hundred about 3½ parts of ashes, which are composed for the greater part of carbonate of lime, a small quantity of iron, phosphate of lime, and perhaps also a minute portion of alkali.

2. Ten

2. Ten grammes of white gum adracanth, subjected to the same experiments, yielded three decigrammes of ashes, which were composed of the same principles as those of the red gum; besides, by washing them, a small quantity of alkali was obtained, namely potash.

3. Ten grammes of gum arabic, burnt like the others, left three decigrammes of ashes, which were composed of the same principles as the preceding, with this difference that they exhibited no signs of the presence of alkali or of sulphur, as the others did.

I had formerly suspected that the opacity of gum adracanth and the difficulty of its solution in water, were owing to the existence of a larger proportion of earthy matter; but from these experiments it appears that they are to be attributed to another cause.

But in what state does the lime exist in the gum of which I have been speaking? Undoubtedly not in that of carbonate from whence I obtained it, and still less in that of quick lime; for the solutions of gum are not alkaline, but on the contrary slightly acid. At least when we rub a piece of gum upon wet paper, it is sensibly reddened. It is also certain that the oxalate of ammoniac and the carbonate of potash produce precipitates in the solution of gum arabic, and that the acetate of lead forms none in it. Hence it follows, according to all appearance, that lime, in the gums, is combined with an acid, but what is this acid?

Here for want of facts I shall be obliged to indulge in conjecture, but conjecture which has great probability, which every thing seems to support, and nothing to contradict. It cannot at least be doubted that it is a vegetable acid with which the lime is combined; for they alone, in their decomposition, leave their bases combined with carbonic acid.

This

This being admitted, let us see which among the great number of these acids is that which can best fulfil all the conditions. It is neither the oxalic, nor the tartarous, nor the citric, since their combinations with lime are insoluble in water, besides which they exist only in a very small number of vegetables; still less can it be the benzoic, the gallic, the *moroxali* or the *komistic* acids; for these, it is known, are very rare in nature, and the three latter form also salts of very little solubility.

The choice is therefore only left us between the acetous and the malic acids, which are the most generally diffused in the vegetable kingdom. The first, we know, forms soluble combinations with all the known substances with which it is capable of uniting; some of them even are deliquescent. Moreover it is the most frequent result of the operations of nature in the vegetable and animal system, since it is formed by vegetation, fermentation, the action of the powerful acids, and the influence of heat.

The combinations of the second are for the greater part insoluble in water; that which it forms with lime particularly is not sensibly soluble except with the aid of an excess of acid, and its existence in nature is not so frequent as that of the acetic acid; and as the lime which is found in the transparent gums has indubitably been dissolved in the juices of the vegetables which furnish these substances, it is much more probable that this earth is combined in them with the acetic acid than with any other.

It is very probable also that the small quantity of potash which I have found in the ashes of the burnt gums, is combined with the same acid; which would explain why these substances are so sensible to moisture, and soften in such a manner that they can no more be pulverized.

I am

I am however much inclined to believe that in certain opaque adracanth gums, which are difficult of solution, and which yield much lime by incineration, this earth is combined with malic acid. I have lately had occasion to examine a gum collected by M. Palisot-Bavois upon the cochineal plant, which was opaque, swelled up in water, but did not dissolve in a homogeneous manner, and which yielded 8 *per cent.* of lime. As the sap of all the cactuses which I have subjected to analysis has yielded me more or less considerable quantities of acidulous malate of lime, it may very reasonably be presumed that that species which nourishes the cochineal insect contains it also, and that it is this salt proceeding from the vegetable, dissolved in the sap with the gum, which gives it opacity and prevents its solution in water.

It results at least from these experiments, that the gums contain; 1, a calcareous salt, most frequently acetate of lime; 2, sometimes a malate of lime, with excess of acid; 3, phosphate of lime; 4, finally, iron which probably is united also with phosphoric acid.

Experiments on the Eudiometrical Means, and on the Proportion of the Constituent Principles of the Atmosphere.

By Messrs. HUMBOLDT *and* GAY-LUSSAC.

From the JOURNAL DE PHYSIQUE.

IF philosophers and chemists are at present agreed respecting the nature of the constituent principles of the atmosphere, they are not yet so with regard to their absolute quantity. Since Scheele and Lavoisier, who had found twenty-seven hundredths of oxygene in the air, numerous experiments, for which we are indebted to Messrs.

Messrs. Cavendish, Marti, Berthollet, Fourcroy, and Dery, have greatly modified this proportion, by fixing it at between twenty and twenty-three hundredths. Nevertheless it must be admitted that these proportions are still far remote from that degree of accuracy which the present state of Science allows of; or, if these limits are well established, it must be concluded that the atmosphere experiences considerable oscillations in its composition. Although with respect to most chemical phenomena, it is not necessary that we should have an accurate knowledge of the absolute quantity of its principles, this knowledge is not less interesting in itself, than important with regard to the history of our globe. If all the facts of geology tend to prove that the earth is no more what it has formerly been; that the highest mountains have been covered with water, and that the Arctic regions have nourished animals which at present are to be found only between the tropics, these very changes prove of what great utility it would be to future ages actually to ascertain the physical state of the globe at the present day; and even though the grand catastrophes which it has already experienced should never more be reiterated, it is possible that it may undergo gradual modifications which man would not be able of himself to appreciate, unless he found incontestible documents in the annals of science. It would therefore be of the highest importance to fix in an authentic manner the grand phenomena of nature, which are to be supposed subject to variation; such as the intensity of the magnetic forces, the elevation of the barometer at the level of the sea, that of the sea itself, the medium temperature of every climate, and the proportion of the constituent principles of the atmosphere. We have directed our attention to the latter question, and although we have not yet solved it in a manner entirely satis-

satisfactory to ourselves, we venture to give an account of the commencement of the investigation which we have undertaken upon this subject, and the researches to which it has led us.

But the eudiometrical means which must serve to determine the proportions of the constituent principles of the air are not all susceptible of equal precision, and some distinguished chemists give the preference to a means which is rejected by others. It therefore was indispensably necessary that we should put to the trial the known eudiometrical methods, in order that we might be enabled properly to appreciate their value; for we are well convinced that accuracy in experiments depends less upon the faithful observation of the divisions of an instrument, than upon the accuracy of the method itself. Although the nitrous gas appears indeed at first to be the most uncertain eudiometric means that can be chosen, we have convinced ourselves that by combining its action with that of the sulphate of iron, or of the oxygenated muriatic acid and of potash, it is capable of indicating with great precision the quantity of oxygene contained in the air. All the eudiometrical means would give the same results, if we were equally acquainted with them all, and it is only because it is very difficult to make all the corrections they allow of, that we naturally give the preference to such as present less occasion for them, although they are not always the most simple in their use. We shall therefore first give an account of the eudiometrical researches with which we have occupied ourselves, and then apply them to the analysis of the atmospheric air and of that obtained from water under different circumstances, or placed in contact with it. We find it necessary again to repeat that we shall not treat the question which we have proposed to ourselves in so extensive a

manner as it merits. Compelled to break off our researches before we were able to bring them to their termination, our purpose is only to give an account of their principal results. During nearly two months since the time that we commenced them in one of the laboratories of the Polytechnic-school at Paris, we pursued them, in spite of the coldness of the weather, which is very disagreeable in researches of this kind, with so much the greater assiduity, as M. Humboldt found a peculiar interest in them. In the year 6, he had presented to the Institute two memoirs on the analysis of the air, which contain a great number of experiments, which he now considers (it is he himself that avows it) not only as very inaccurate, but also as very justly controverted by Mr. Davy, and by a chemist with whose particular friendship we are both honoured, namely, M. Berthollet. Zealous for the advancement of science, M. Humboldt, when he commenced his researches, wished to substitute in the place of those investigations of his early youth, others founded upon more solid foundations, and as he desired to have me for his associate in them; I ought to consider myself the more highly honoured by his proposition from our having been, since his return from the tropics, linked together in the closest bonds of friendship.

Observations on several of the Eudiometric Means.

It is not our intention in this memoir to record all the researches which we have undertaken upon different eudiometric means: most of them are still in too imperfect a state; but having occupied ourselves more particularly with the alkaline sulphurets, and especially with hydrogene gas, we shall here exhibit the result of our observations on these two eudiometrical means.

Although

Although the alkaline sulphurets have in general a sufficiently-constant action for the analysis of the air, which has caused them to be justly preferred to the other eudiometrical means, they nevertheless present some sources of error, which it is indispensibly necessary we should be well acquainted with, in order to justify us in placing an entire confidence in their results. It has long been believed that they have no action upon the azote; and although M. Marti announced, as early as in the year 1790, that they absorb this gas, no farther attention has since been paid to this property. It is true M. Marti at the same time announced, that by saturating them with azote we might employ them to advantage for the analysis of the air, and constantly obtain for the oxygene a proportion comprehended between 0.21 and 0.23. On the other hand, this chemist not having indicated the details of his experiment with sufficient precision, M. Berthollet, who repeated it after him under different circumstances, announced in his Chemical Statics, that he had not found the alkaline sulphurets to possess the property of absorbing azote, whereby he gave new sanction to their employment for the analysis of the air. When we began to employ this means we placed great confidence in it, and had nothing to object to it but the great length of time which it requires, and which had long rendered it a desirable object that, notwithstanding its accuracy, some other might be substituted in its place that were not attended with the same inconveniences; but we soon discovered that it did not always act in an uniform manner, and herein we were favoured by accident.

Having placed 100 parts of atmospheric air in contact with a solution of sulphuret of potash, made with hot water, in three vessels of unequal capacities, we observed at the end of eight days that the air had lost twenty-three parts of its volume in one of the vessels, and 23,6 26,0

in the two others. This great inequality gave us at first much surprise; but having remarked that the absorption had been most considerable in the largest flask, we suspected that azote had been absorbed, and, in order the better to confirm our suspicion, we repeated the same experiment, employing two vessels more unequal in capacity, but otherwise placed in the same circumstances. At the end of ten days we found that in the small flask the absorption had been only 22,5 parts, whilst in the large one it was 30,6. But the most decisive experiment which we made on this subject, was the placing of a solution of sulphuret of potash, which had been heated to ebullition, in contact with azote in unequal vessels, when it was discovered that the absorption was proportionate to their capacities. It would therefore be possible to cause a determinate quantity of atmospheric air to be absorbed by a solution of alkaline sulphuret, and to make it be considered as pure oxygene, if it were supposed that the whole diminution of volume were owing to the oxygene gas. But if, instead of employing a solution of sulphuret made with hot water, we employ one made with cold, as M. Berthollet has always done, the solution of the azote takes place no longer, at least not in a perceptible manner, and the results of the analysis of the air made by this means become then much more susceptible of comparison. This variable action of the alkaline sulphurets, dissolved at different temperatures, requires to be farther elucidated, which we are about to do by citing phenomena of an analogous but more comprehensible nature.

As water always holds in solution a certain quantity of air in which the proportion of oxygene is more considerable than it is in the atmospheric air, it happens that when we heat it, or dissolve a salt in it, a part of its air is disengaged from it whilst another is retained, which may

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be separated from it by a more intense heat. If, therefore, we place this water, which has been deprived of its air by the last-mentioned means in contact with atmospheric air, it will absorb, in returning to its original temperature, a quantity equal to that which it has lost; and if we were not aware of this absorption, but judged upon appearances, we should suppose that the water alone, or charged with salt, had made the analysis of the air. Thus M. Hetter has very recently announced that a solution of muriate of soda absorbed all the oxygene of the air, though on repeating his experiment with a highly-charged solution of the same salt, but made with cold water, we did not find the slightest difference between the ordinary atmospheric air and that which had been in contact with the solution of muriate of soda for a month and a half.

The very same thing happens with a sulphuret as with a salt. At the moment of its solution in water, a portion of air is expelled, and an equilibrium of saturation is established between the water, the sulphuret, and the air which it holds in solution, so that if the circumstances are not changed, there is now no reason why it should absorb a fresh quantity of air; but if we heat the solution, there is disengaged from it a part of the gas which it contained, and, in returning to its original temperature, it is necessary that it absorb what it had lost, in order that its equilibrium may be re-established *. We

* The absorption of which we mean to speak in this place is independent of that of oxygene by the sulphuret, which is thereby converted into sulphate. But as the sulphuret absorbs the oxygene which the water holds in solution, it will very probably happen that the water will be able to absorb a larger quantity of azote; so that if we employed a solution made with cold water, and very recently prepared, there would be a still greater diminution of volume than that which is owing to the absorption of the oxygene. We say *very probably*; for we have not yet made the experiment.

think,

think, therefore, that the difference between the results of Messrs. Marti and Berthollet may be explained by the difference of the circumstances under which they operated; but it appears to us, that M. Marti believed that it was the nature of the sulphuret to absorb azote, whereas it does not absorb it all, but rather prevents the water with which it has been boiled from absorbing as much as it would do without it.

Thus if we take care to dissolve the sulphurets in cold water, and leave them for some time in contact with azote or with air, we may employ them with advantage for the analysis of the atmosphere. We shall however observe, that as they are attended with the inconvenience of requiring a great length of time before their action is completed, we are obliged to have recourse to the corrections of the thermometer and barometer, which often are very uncertain. The best way of remedying this inconvenience is undoubtedly to follow the method of Messrs. Berthollet and Marti, which consists in placing, for comparison, upon water a determinate quantity of air, in order to judge, from its variations of volume, of that of the air which we analyze; but this method has not appeared to us to be attended in practice with all the advantage which it seems to promise.

We must still remark with regard to all the eudiometrical means, where the absorbing substance is solid or liquid, that if we commit an error, either in observing the divisions of the instrument, or in the appreciation of the uncertainties of the method, this error necessarily falls altogether upon the quantity of oxygene; and since with all possible exactness we cannot answer for its amounting to much less than a hundredth part, it would follow that the proportion of oxygene contained in the air cannot be determined within this quantity. In fact, we find that chemists who have employed such means have

have found very considerable variations in the quantity of oxygene of the air; and M. Marti himself, who appears to have made a great number of experiments with the alcaline sulphurets, and who was acquainted with the precautions which they require, fixes it between 0,21 and 0,23. We shall see hereafter that the eudiometrical means, in which the substance which combines with the oxygene is gasceous, admit of a much greater precision.

As we had proposed to ourselves from the commencement of our investigation, to ascertain whether the eudiometer of Volta was fit to be employed for the analysis of the air, we principally directed our attention to it. This instrument has been accused of being fallacious, of indicating too small quantities of oxygene in the air; but it appeared to us that supposing it required corrections, we might by appreciating them, as well as the law of their variations, render it very exact and convenient. With this view we proposed to ourselves the following questions:

1. When a mixture of hydrogene gas and oxygene gas is inflamed in Volta's eudiometer, can the absorption of one of the gases be complete?

2. Is the product of their combination of a constant nature?

3. What is the exact proportion of the two gases for forming water?

4. What are the limits of error that Volta's eudiometer admits?

We must examine these four questions in succession, but first of all we think it incumbent upon us to give an account of the manner in which we prepared the gases which we employed in our experiments.

TO BE CONTINUED IN OUR NEXT.

Muriatic

Muriatic Solution of Tin in part decomposed into Crystallized Metallic Tin. By M. BUCHOLZ.

From VANS MONS JOURNAL DE CHIMIE.

WITH a view to prepare the muriate of tin, I heated 7 lbs. of the finest English tin with 15 lbs. of muriatic acid, of 1,120 sp. gravity. At the close of the evening there remained still from 2 to 2½ lbs. of tin to be dissolved. On the following day the matter was still warm, and the liquor had the consistence of a syrup. I gently poured upon it a pound of water, which swam above the solution. At the end of an hour, on examining this mixture, I was surprised to find that the undissolved tin, and particularly its projecting parts, were covered with points, needles, laminæ, &c. of crystallized metallic tin, from ¼ to ½ an inch in length.

On repeating the same operation some time after, I had the satisfaction to see the same phenomenon reproduced. I convinced myself, by all possible trials, that the tin which I had employed was perfectly pure.

List of Patents for Inventions, &c.

(Continued from Page 160.)

JOSEPH STEEL, of Stockport, in the county of Chester; for a species of cloths, fustians, callicos, cambricks, lawns, striped cottons, and other articles, manufactured with cotton, wool, and flax, mixed and spun together. Dated December 17, 1805.

THE

REPERTORY

OF

ARTS, MANUFACTURES,

AND

AGRICULTURE.

No. XLVI. SECOND SERIES. March 1806.

Specification of the Patent granted to JOSEPH HUDDART, *of Highbury Terrace, in the Parish of Islington, in the County of Middlesex, Esquire; for sundry new Improvements in the Manufacture of large Cables, and Cordage in general.* Dated October 30, 1805.

With a Plate.

TO all to whom these presents shall come, &c. NOW KNOW YE, that in compliance with the said proviso, I the said Joseph Huddart do hereby describe and ascertain the nature of my said invention, and the manner in which the same is to be performed, as follows; that is to say: These improvements have for their object the laying of cables and cordage by the machinery exhibited in the annexed drawing, which will be intelligible on inspection by rope-makers, and others conversant in mechanism. Such persons will of course understand which of the wheels are intended to have teeth in them, and will suppose the necessary supports or frame-work for the machinery represented, these being omitted in the

drawing with a view to the more clear and distinct exhibition of the other parts.

I do not allege that any single thing represented in the drawing above referred to is in itself new, or that the particular combination of any part or parts thereof, with any other part or parts thereof, is new, supposing the same to be separately taken and considered. But what I lay claim to as my invention in these improvements, is the ultimate combination of these several parts, as forming one intire machine, or system of machinery, tending to the laying of cables and cordage in a manner that is to all substantial and essential purposes new.

The following description will assist those who may want instruction beyond what the bare inspection of the drawing above referred to will furnish.

I propose to make use of some parts of the machine described in the specification of a patent, granted to Mr. Edmund Cartwright, about May 15, 1792, under the name of the *Cordelier*.

In the annexed drawing A, Fig. 1, (Plate X.) is the axis, B the top fixed to it as a gudgeon, with holes therein for the strands to pass through. C and D are circular planes or tables fixed to the axis, and connected together by three or more pillars, which are not expressed in the drawing. E E are two similar jacks, or spole-frames, of which as many are to be used as shall be equal to the number of strands of which the cable is to consist, being most commonly three. Those in the drawing are represented in different positions; by which means the several parts are the more distinctly seen. F is a dead or fixed wheel, within which the axis revolves, and the teeth of which connect with those of G G, which are counter-wheels upon the circular plane or table D, and the said G G connect also with H H the jack or spole-

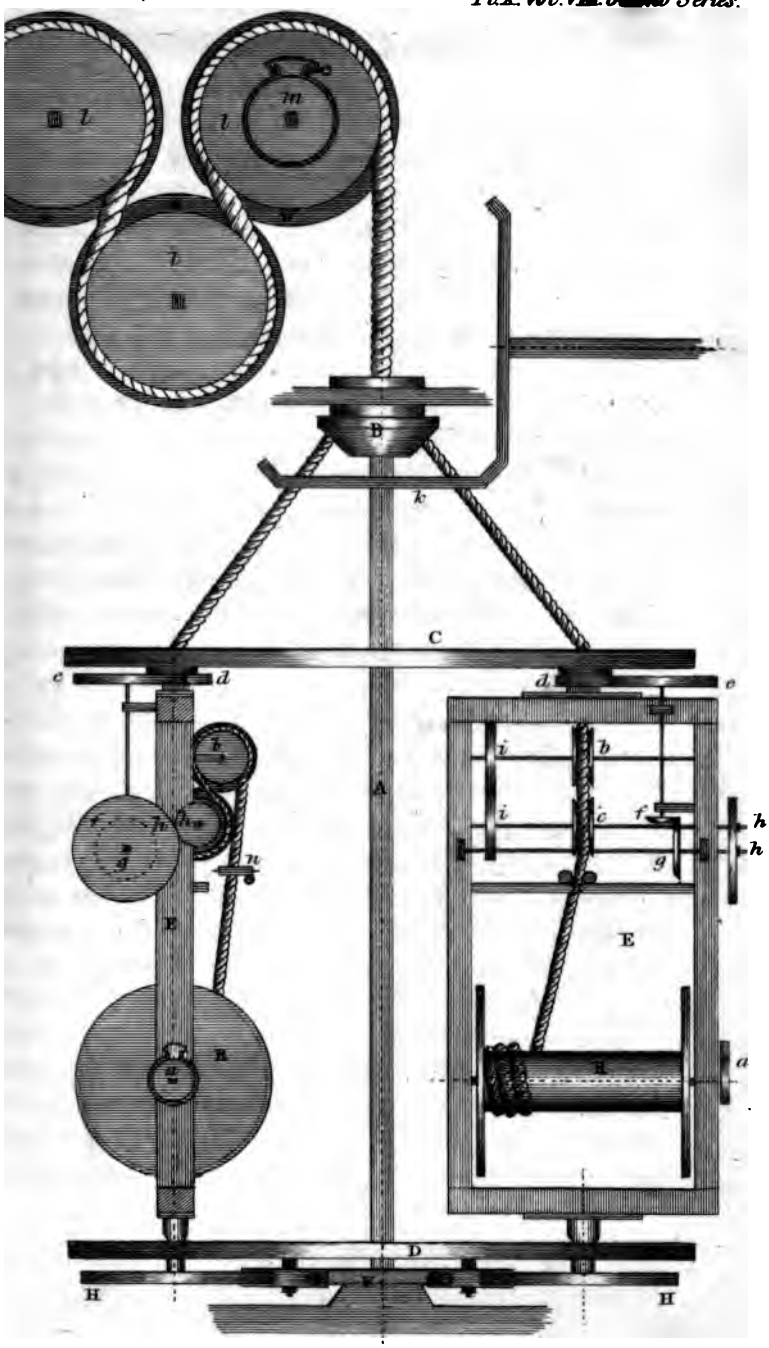
spole-frame wheels. The above-described parts, although not similar to Mr. Cartwright's construction, are intended to produce the same effect, and therefore may perhaps be considered as part of his invention. The strands I presume to be previously prepared by means of the register which I invented and described in the specification of a patent granted to me in 1793, which I call registering, in order to produce a more equal distribution of the strain upon the yarns, as without such preparation no machinery whatever can in my opinion lay the first strands of cordage so well as by the old or common mode. The strands are to be wound upon the reels R R. Friction blocks or clutches (*a*) (*a*), connected with the axis of each reel, are fixed to regulate the tension of the strand on its being drawn from the reel. The friction block or clutch is applicable to many useful purposes invented by me, and described in my inrolled specification, dated about the 28th of July, 1800. From the reel in each spole-frame the strand passes through a guide, to direct it into the plane of the groove of the two whirls *b* and *c*, which guide is provided with the friction-roller (*n*). Those whirls being grooved, and in all respects equal to each other, and having equal motion given to them by the two equal wheels *i i*, and by the strand winding round them, as represented in the drawing, will deliver equal quantities in every revolution without slipping, and without bruising the strands. The counter wheels G G, above mentioned, are to give to the spoles, with the strands, a contrary motion to that of the axis A, and will thereby produce a relative motion between the plane or table C and the spoles; and, in order to regulate the motion of the whirls *b* and *c*, I have fixed to *c* the wheels *d d*, in which the hollow gudgeons of E (the spoles) revolve, and those wheels communicate the motion to the planet

wheel *e e*, and the bevil pinions *f f*, and the wheels *g g*, and to the shifting wheels *h h*, which last may be varied in their dimensions and number of teeth, according to the size of the rope, to regulate the motion of the whirls *b* and *c*, which are carried by the shifting wheels, and which two whirls have, as before mentioned, equal motion, by reason of the equal wheels *i i* upon their shafts; or, instead of the shifting wheels *h h* being changed, the construction or size of the whirls may be varied according to the size of the rope. By these means, and by having the wheels, whirls, &c. respectively equal and similar in each spole, equal quantities will be delivered in every revolution from each spole. From the whirl *c* the strand passes in the line of the axis of each spole through the gudgeon, which is perforated as in the spole of a spinning-wheel, and through the wheel *k*, fixed to the axis A, and also through the perforated top or gudgeon B, where the strands are formed into a rope or cablet.

The application of the whirls *l l l*, which are also carried by the equal wheels *w w w*, and a friction block or clutch (*m*) upon the axis of one of them, are of peculiar use to regulate the tension upon the rope or cablet. And the shafts of those whirls being by means of the said friction block or clutch connected to a shaft from the power, the tension may be more accurately adjusted than by weights upon the sledge, as in the common mode of laying cordage.

In witness whereof, &c.

Specification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Specification of the Patent granted to DANIEL DESORMEAUX, *of Barking, in the County of Essex, Surgeon and Apothecary, and* SAMUEL HUTCHINGS, *of Ilford, in the said Parish of Barking, Weaver; for Improvements in the making and manufacturing of Wax, Spermaceti, and Tallow Candles.*

Dated October 22, 1805.

TO all to whom these presents shall come, &c. NOW KNOW YE, that in compliance with the said proviso, we the said Daniel Desormeaux and Samuel Hutchings do hereby declare, that our said invention is described in manner following; that is to say: Instead of the wick usually made, applied, and used, in the manufacture of candles, which consists of a loose rope, cord, or strand of cotton, or other suitable yarn, composing what may be called a solid cylinder, we construct and use a wick, in which the threads or yarns are so disposed and connected with, or applied to, each other, as to form a tube or hollow wick, by which means the external surface of our said wick is rendered much greater than the external surface of a common wick, consisting of the same weight of yarn, or number of like threads, and various other considerable advantages are obtained. And we do fabricate or manufacture our said wicks either by drawing the threads parallel to each other in sufficient number through holes in a lucet, or other similar implement, and by smearing them, while thus disposed along side of each other, with some glutinous inflammable matter, such as wax, or as tallow mixed either with wax or resin, in due proportions, to be ascertained by trial, by which means a flat and manageable wick is formed of any desired width, and may be cut into suitable lengths, and used as hereinafter

hereinafter directed. Or otherwise the said flat piece may be fabricated and made by weaving, or by any other known method. Or otherwise we do weave, or do fabricate and make, by any other known method, certain tubes of cotton, or other fit material, of any required dimensions, weight, and length. In the manufacturing of candles with our said improvements, we do apply or dispose the flat wick, hereinbefore described, upon a wire, so as to wrap round and cover the same, with its length nearly in the longitudinal direction of the wire; and we secure the said wick in its place by tyeing it round the end, or by sliding a perforated small piece of wood or button on the upper end, and by tyeing it round at the lower end so as to embrace and confine the same, or by any other fit means to prevent the wick from sliding off the wire. And in the subsequent processes of manufacture we do cover or envelop the said wire and wick with wax, spermaceti, or tallow, by dipping the same in the melted material, or by using suitable moulds, in the same manner as the ordinary wick is covered or enveloped in the making of common candles. And when the said candles so manufactured, and containing the improvements hereinbefore described, have become cold and hard, we hang them singly up, each by its wire, from which we do then separate each candle severally, by drawing the same downwards, and the candle is then seen to be longitudinally perforated, either quite through, or within a small distance of the bottom, according to the manner in which the process may have been intentionally managed. And farther, we do likewise manufacture candles by applying a suitable length of the tubular wick upon a wire properly fitting the same; and we secure the said wick in its place by the same means as are hereinbefore directed to be used with the flat wick. And we do then

proceed

proceed in making and completing the candles in the same manner as before directed for making the said candles with our said flat wick. And farther, we do declare that, according to the uses and purposes for which the said candles may be intended, we do make our wicks of greater or less diameters in proportion to the weight of wax, spermaceti, or tallow, made use of in each candle; and we do vary the relative quantity of cotton, or other fit material, and the fineness of the threads thereof in the said wicks, the particulars whereof must be left to the choice of the maker and consumer of the article, and cannot therefore be here pointed out. And farther, we do in some cases apply a coating of wax, spermaceti, or tallow, more or less thick, to the inner surface of the hollow cylindrical wick, either by dipping or by a mould with an interior taper, wire, or core, or by various other means, sufficiently obvious to those who are accustomed to works of the nature of those herein described. And, in other cases, we do entirely fill up the interior cavity after drawing out the wire, or by any other suitable management. And, in other cases, we make the external part of the said candles of wax or spermaceti, and the internal part of tallow, or either or any of the mixtures of the said materials. Lastly, with regard to the interior cavity, we in some cases (such as when the diameter is considerable) leave the lower part open, to admit an interior current of air. And in other cases (such as when the diameter of the wick is proportionally small) we make the candles with the lower part closed, and resembling in that respect the candles commonly made.

In witness whereof, &c.

Specification

Specification of the Patent granted to THOMAS JAMES PLUCKNETT, *of Deptford, in the County of Kent, Gentleman; for an Addition and Improvement to Capstans and Windlasses for Ships, and other Purposes.*

Dated February 25, 1801.

With a Plate.

TO all to whom these presents shall come, &c. NOW KNOW YE, that in compliance with the said proviso, I the said Thomas James Plucknett do hereby describe and ascertain the nature of my said invention, and in what manner the same is to be used and performed; that is to say: The improvements I have made to the capstan and windlass apply to the surging or forcing up the messenger or rope going round the capstan, and the fleeting over, or forcing over, the cable or rope going round the windlass; and also to the holding on, or hauling off, the messenger or rope going round the capstan or windlass.

As to the first improvement which is intended to facilitate the winding on and off the messenger or rope going round the capstan, in other words, to avoid the necessity of surging or forcing up the rope by beating it up with a maul, or other instruments, or by slacking it, to let fly up the whelps, when there is a great strain on the capstan, which operation is often attended with great danger, and always loss of time. This I do by means of pieces of iron, of about two inches square, more or less, and from twelve to eighteen inches long, according to the size required, which I call lifters, (see drawing annexed, Plate XI, Figs. 1, 2, and 6); and which lifters are carried round with the capstan, and working on a circular wheel or rim in a groove in the deck, which, by

reason

reason of its being formed upon the principle of an inclined plane, I call an inclined plane, see drawing annexed Fig. 3, and letter A of Fig. 4. These lifters C C C, of Fig. 4, are placed in the cavities between the whelps B B B B, of Fig. 4, and are secured either by going through sockets or holes in the paul wheel, or otherwise, so as to work perpendicularly up and down on the inclined plane as the capstan goes round. The head of the lifters I make as long as the distance between the whelps and the transverse of them, no broader than to the edge of the paul wheel; or when there is no such wheel, then to be about three, four, or five inches, according to the size of the capstan, farther out than the whelps, with the outer part thereof rounded down, for the convenience of rising under the rope before it reaches the whelp. To the bottom of the lifter is prefixed a friction roller, which travels round on the surface of the wheel or rim, which, as I said before, I call an inclined plane. This inclined plane, upon which the friction rollers of the lifters run, must be formed in a circle, and fixed in the deck under the capstan, so that the friction rollers of the lifters may traverse round immediately upon it. Therefore the inclined plane must present a surface of breadth sufficient to receive the friction rollers at all times, and being lower at the larboard and starboard sides of the capstan, and gradually rising to the aftermost and foremost part thereof, producing a rising upon the principle of the inclined plane, so that, by the lifters travelling round thereon as the capstan goes round, the head of them rises under the messenger or rope, and thereby gradually forces it up, so as always to let in, or make room for, the succeeding turn of the rope to come upon the capstan without a possibility of its ever riding or coming over

the former turns, whether you heave the capstan round with or against the sun. This hoop, forming the inclined plane, instead of being fixed as above, may be made moveable and thicker at one part of the circle only, gradually descending to the opposite part, or semi-diameter thereof; and in that case it must be shifted round, as occasion requires, so that the lowest part of this inclined plane may be at that part of the capstan where the messenger or rope is winding. The said lifters and friction rollers to be made of iron or brass, or other metal or materials sufficiently strong to bear the stress or weight they may have to sustain. The principle above described, as to the inclined plane and lifters with friction rollers for the capstan to avoid or assist the surging, I apply to the windlass also, having hoops or wheels upon the same inclined plane principle and lifters, with friction rollers working thereon, to prevent the necessity of forcing the cable towards the ship's side, called fleeting over. As the windlass works round upon an horizontal axis, the lifters will also move in an horizontal direction at right angles from the inclined plane, and the base of the inclined plane at right angles from the axis of the windlass. A wheel or inclined plane, such as I have before described, must be fixed on both sides of the paul-bit, for the convenience of working or getting up the larboard as well as the starboard anchor, with a set of lifters to each; the number of which lifters will be from three to six, according to the size of the windlass. These hoops or inclined planes may be made thick only at one part of the circle, going gradually thinner to that part of the circle which is opposite to the highest; and the lifters being secured by means of a frame, fixed to the windlass for the purposes of forming sockets for them to work

work in, they will then operate upon the cable or rope round the windlass upon the same principle as they do upon the messenger or rope going round the capstan, as before described. Now, in order to draw the lifter back to the inclined plane after having been forced out by the rollers running thereon, (its position being horizontal,) I place a small friction roller to each lifter projecting beyond the outside of the lifter, see drawing annexed, Fig. 5, letter A, which is to travel round in a groove made in the side of the inclined plane, parallel to the surface thereof, letter B; which small friction roller being confined by the upper part of the groove, the lifter must necessarily be kept upon the inclined plane. As the windlass goes round, the head of the lifters, each in their turn coming against the cable, always receiving it as it winds on, must force the cable on, and, by working in the inclined plane, always keep it in a position to let in the succeeding turn thereof; so that no fleeting over is necessary, and consequently no delay in weighing anchor, or delay when the windlass is used for other purposes.

As to my farther improvement, which applies to the holding on, or hauling off, the messenger or rope going round the capstan, or round the windlass, I describe as follows: I make a solid wheel with cog-work, called a trundle, (see drawing annexed, Fig. 4, letter A 2,) and a spindle fixed through the centre of it, secured on the deck above and below B 2, B 2, forming an axis for it to work on.

The socket holding the axis of the trundle I make in the form of a groove, in order to adjust the distance of the trundle from the whelps of the capstan, otherwise the spindle being confined at a certain distance from the

capstan, it would only answer for one size rope; but, by means of the groove, the distance of the trundle from the whelps may be adjusted so as to answer for any size rope which will be nipped by the aid of a screw or lever pressing the trundle towards the capstan. This trundle is to turn on its axis by the motion of the capstan, by means of the cog-work, acting with other cog-work, fixed upon the body of the capstan under the drum-head; and these cog-works playing one into the other carry the trundle round on its axis; and the socket or groove at the upper end of the spindle being made to force the spindle towards the capstan by the screw or lever, you bring the trundle to the capstan, according to the degree of pressure necessary upon the rope round the capstan to assist the holding on, or prevent its rendering.

In order to apply this last improvement, the body of the capstan must be made solid, without any cavity between the whelps, otherwise the rope to come in contact with the trundle would not receive the pressure of it. The cog-work should be made of iron, or other metal or materials sufficiently strong for the purpose. The diameter of the trundle must be governed by the diameter of the drum-head and paul-wheel.

This last description is for the double capstan, when the power is applied to the upper body, and the rope winds on upon the lower body. But when it is applied to a capstan where the rope is brought to the same body where the power is, or where the men heave at, then the upper end of the spindle of the trundle must be secured in a frame coming from the deck below, of a height to admit the bars or lever to pass freely over it.

In witness whereof, &c.

Specification

Fig. 1.

Fig. 2.

Fig. 5.

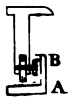


Fig. 6.

Fig. 3.

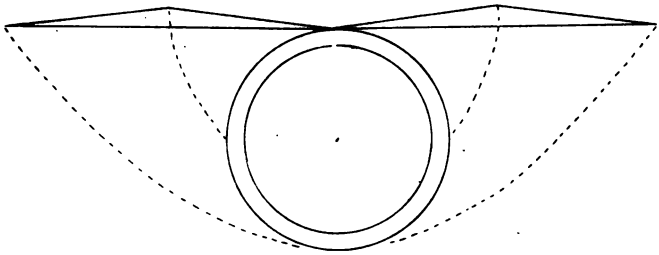
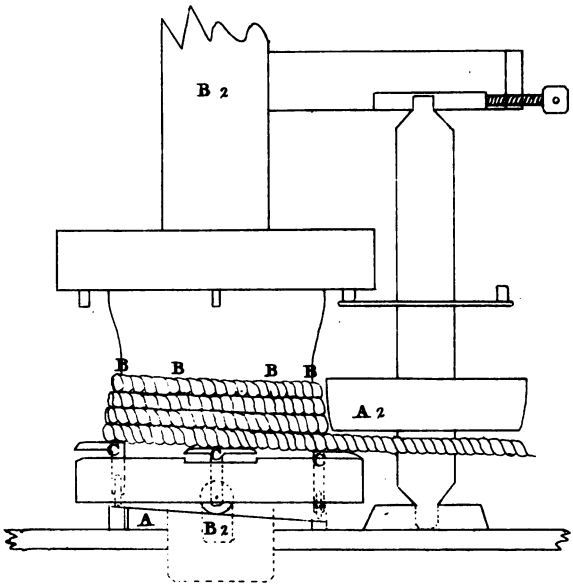


Fig. 4.



Specification of the Patent granted to ABRAHAM OGIER STANSBURY, *of the City of New York, in the United States; for Locks and Keys upon an improved Construction.* Dated May 18, 1805.

With a Plate.

TO all to whom these presents shall come, &c. NOW KNOW YE, that in compliance with the said proviso, I the said Abraham Ogier Stansbury do hereby declare, that my said invention is described and ascertained by the annexed drawing, and description thereof.

In witness whereof, &c.

Before I proceed to describe my invention, it may be proper, by way of introduction, to say something of the nature of locks generally, and point out wherein their excellence consists as applied to the security of property. Locks are the guardians of our most valuable treasures, the silent protectors of property, to whose fidelity we entrust not only our fortune but our lives; hence it is of importance to adopt such a construction as will admit of any required multiplication of numbers for the public use, without the necessity of making two alike: this is one point of consequence. The next to be considered is strength, to render the lock secure from violence; in order to this, the bolt should be large in proportion to the size of the lock, and the parts used to retain the bolt likewise require to be compact and firm, that they may not be liable to derangement. A third qualification of importance is, the security from picking: this is obtained by preventing the access of any thing to release the bolt except its proper key. For this end, the best construction is that which opposes the greatest difficulty to the making of a false key, for it is of little consequence what

variety

variety of keys the plan admits of, if a false one may be made from an inspection of the lock itself. A farther object of consequence is, the avoiding of friction as much as possible, that the bolt may move with facility, and the works may not be injured from use. That part of door-locks called the catch, or catch-bolt, which is thrown out by a spring to hold the door shut, being in constant use, requires more particularly to move with ease, and to be so constructed as not to be liable to get out of order, which in the kind generally used is very often the case. In my invention, security, simplicity, and strength, have been my principal aim, as they appear to comprize all that is valuable in a lock. The principle of strength, is that of pinning or nailing two pieces or plates of metal together while their surfaces are held in contact, for it is well known, that if even two boards be fastened together with a single slight nail driven through them, it will require an immense force to separate them by sliding. For security, I make these nails or pins of such infinite variety, from their position and number, as to admit of any required number of locks without having two alike. In addition to this, I multiply the varieties of position and number by every change in the thickness of the moveable plate; and, as a farther source of variety, the pins may be cut into unequal parts, by which means they will require to be pressed to different depths from the point of bisection, in order to release the moveable plate. A similar variety is produced by having grooves in the moveable plate, and corresponding raised parts on the fixed plate, so that the extent of changes afforded by a combination of these varieties becomes absolutely incalculable. In order effectually to prevent the opening of the lock by any thing but its own key, the nails or pins may be placed out of sight, and guarded

guarded from access by the projecting stud of the lock on which the key turns; and as a security against the taking of an impression of the pins or nails by the insertion of a soft substance or blank key, &c. I make a number of marks on the surface of the moveable plate, exactly similar to those caused by the ends of the pins entering through it. These pins admit likewise of being made of various sizes or diameters, so that some of them may be extremely small, and by this means render it nearly impossible to make a false key even from the nicest inspection of the real one. Thus the difficulty of ascertaining the number and position of the nails is alone sufficiently great without resorting to any variation of their relative lengths, and the accuracy required to make a key, whose points shall strike on all the pins or nails at once the precise line of division between the surfaces of the two plates in perfect contact with each other, is greater than can be readily conceived, and can only be effected by taking the lock to pieces and making the moveable plate itself the gauge for the points on the key. The pins or nails entering through the fixed plate are thrown by springs into the moveable plate, and the business of the key is to push them back by corresponding points fixed in it, so as to release and turn the moveable plate without entering the fixed plate; for were this the case, these points on the key would act as nails from the moveable into the fixed plate, and effectually prevent the release of the bolt. The bolt may be made either separate from the moveable plate, and so moved by a tooth or teeth, or the plate itself may constitute the bolt or fastening. These methods are applicable to locks of every description, as padlocks, door-locks, chest and desk locks, &c. &c. as will appear more fully from the drawings given. Any required number of bolts may also be

thrown

thrown in different directions at the same time by means of their connection with the moving plate; or the plate itself may be made of such a form (as square, triangular, octagonal, &c.) as to throw out points in every direction when turned by the key; it is applicable to bars thrown across doors into staples, &c. and likewise to window fastenings. In door-locks, where a catch or latch is required, and which in the ordinary construction is released by turning a knob or handle, I make this knob or handle to be pressed directly forward on one side, and pulled directly back on the other, according to the motion of the door, in order to open. This is effected by means of a chain or flexible substance turning, and which, being pushed or drawn, draws back the catch, or releases the latch, and opens the door by the same motion. This pusher may either have a projecting piece acting upon the latch or catch-bolt as a wedge or inclined plane, or it may draw by means of a chain or flexible substance turning over a roller, or the action may be communicated by means of a crank or a wheel, as shewn in the drawings. A spiral or worm-spring is used to throw the latch or catch-bolt; but as this acts in a contrary direction to the motion of the pusher, a second spring may be added, to take upon the pusher and return it; this may be placed either inside of the lock or within a hollow handle fastened to the door, through which handle the pusher moves. Thus by drawing the handle with the fingers so as to hold the door while the thumb presses the pusher, the catch or latch is released silently; on the opposite side it is only necessary to press the door with the thumb, while the pusher is drawn back by the fingers, to produce the same effect; but in ordinary cases a simple pressure of the pusher, without the assistance of the handle, as just mentioned, is sufficient

to

to enter the room. Instead of the small bolt generally used to fasten the door from within, I make the catch-bolt answer the purpose by stoping the action of the pusher with a button placed within the lock, and turning round the pusher. This button is connected to a plate or rim, likewise turning round the pusher in the room, and, as the rim is moved, it turns a projecting part into a notch in the pusher, or under a pin from it. The catch-bolt likewise is made to increase the strength of the fastening, by causing the key-bolt, usually employed alone as a security, to stop the return of the catch, which is thus incapable of being released until the other bolt be unlocked, thus the two together constitute one large strong fastening.

In order to remove the inconvenience of a projecting-box, which is commonly made to receive the bolt and catch of locks placed on a door, and not mortised into it, I make the bolts with an elbow or bend, so as to shoot into the door-post from the middle of the door in the same manner as the bolt of a mortise lock. This mode is also applicable to drawer, desk, and other locks, where it may be used for greater security of fastening by causing the bolt to take farther in; or the bolts may be made straight, and let into the door, which will be equally as neat, and stronger than a mortise-lock.

Having thus given a summary view of the nature of my improvement, I proceed to explain the drawings.

Fig. 1, (Plate XII.) section of the moveable and fixed plates. *a* the moveable, *b* the fixed plate; *s* the stud or centre connecting the plates above and below; *c* a pin or nail, in the position in which it will be when pressed down by the key; *d* a pin or nail thrown through both plates by the spring *e*; *ff* the back of the lock, which must be so near the plate *b* as to prevent the pins

from being pushed out of it by any attempts to pick the lock.

Fig. 2, section shewing the moveable plate grooved, and the fixed plate with corresponding raised parts, the pin *y* pressed to the surface of the plate *b*, and the pins *x* and *z* rising to the surface of plate *a*; *s s* the stud which answers to conduct the key; K the key, having points of the proper length to press the pins or nails to the surface of plate *b*.

Fig. 3, section shewing the pins bisected and passing through the fixed plate *b*, nearly to the surface or top of moveable plate *a*, the holes on the face being smaller than the pins, in order to prevent the upper pieces from coming out of place. These pins may be made with a shoulder, as *c*, or without, as *d*. These pins or nails are cut into unequal lengths, and that next the stud is shewn divided at the line of separation between the two plates, and consequently requires no pressure of the key, which therefore has no corresponding point; this is also the case in Fig. 2. By this mode of bisection, a lock of the most difficult description, having any number of pins, may be made without requiring any increase of the size or complexity of the key, as will appear from Fig. 4, where the key has but two points, and the lock has six nails or pins, four of which are bisected on a line with the surface of bottom or fixed plate *b*, and require no pressure.

Fig. 4, section of a lock to open on both sides; in which case the moveable plate *a a*, instead of turning on a stud, is held at the edge of locks *g g*, the key passing through both plates at the opening or key-hole *h h h*. The key has points at *o* to press as before described, and similar points at *x*, which, being drawn upon the points of the nails, release and turn the plate *a a*. Another way of locking

locking on both sides, is to place two locks reverse to each other, either separate or made under one cover: this can require no explanation.

Fig. 5, A, front-view of moveable plate *a*, used as a bolt, and turned out at *b b* to lock, when the pins beneath rise into their holes at *c*, and hold it fast. *s* stud; wherever the key-hole is represented by dotted lines it is intended to shew its position, not on the moveable plate, but in the face or covering of the lock. B the same plate, with the bolt-part *b b* turned back to unlock, when the holes will be at *c*: and the points of the pins or nails remaining at *i*, under the plate *a*, may be admitted to rise into it by corresponding holes being made to receive them: in which case the bolt or moveable plate is held fast unlocked, and will require the action of the key to lock in the same manner as to unlock it.

Fig. 6, front-view of moveable plate *a*, used as a pinion to turn bolt *b*. *c c*, pins on each side of the stud or centre; K, the key to answer to them.

Fig. 7, A, the moveable plate, to answer as a padlock, without any separate piece or hasp, as is usually required. B the same unlocked.

Fig. 8, the same plan, rendered more secure by exposing less of the arm *a* of moveable plate, which may be either a part of the plate itself, or a piece attached to it; the part B shews the surface of the fixed plate.

Fig. 9, the moveable plate, represented as a bolt for desk or chest locks.

Fig. 10, the moveable plate, shewn as a padlock, having three different rows of holes, into each of which different pins beneath are placed to take as the moveable plate or bolt is turned round. These holes are so situated, that only one row can be fastened at a time, by admitting the pins; all the others remaining behind or

beneath the moveable plate, as shewn more clearly at A, where the holes in the moveable plate are represented by small rings, and the pins beneath by dots. *s* the stud or centre on which the plate moves. These pins being bisected, as shewn in Fig. 3, and each being different from the others, it will require as many keys to release the moveable plate as there are rows of holes. Thus the key corresponding with the holes on the line *s a*, can only turn the plate till the springs throw the pins into the holes on the line *s b*, which is then fixed, and requires another key to turn till the points below enter the holes at *s c*; a third key is then necessary, in order to release the bolt B.

Fig. 11, shews the manner of drawing back a bolt by means of a crank or bending lever applied to the pusher. *a b*, pusher. *c c*, crank turning on the pin *d*. *s* the catch-spring. *k*, the catch or bolt.

Fig. 12, view of a latch raised by a wedge from the pusher. *a*, pusher. *b*, wedge. *c*, roller placed in the latch to diminish friction, but which is not absolutely necessary. *d*, spring.

Fig. 13, pusher, with a roller inserted to answer in place of the wedge.

Fig 14 shews the mode of drawing back the catch by a chain or string over a roller, &c.

Fig. 15, lock of a common form, altered for the reception of pusher, by having a roller fixed on the arm *a*, and an opening made in the plate at *b*, to permit the descent of the wedge: a second roller is shewn at *c*, to support the arm *a*, and diminish the friction of catch-bolt *f*.

Fig. 16, form of a catch-bolt or frame, as shewn in Fig. 19, to admit the key to release the bolt. *a b*, the pusher. *c*, the hollow handle, to receive a spring, as

shewn

Fig. 1.

Fig. 4.

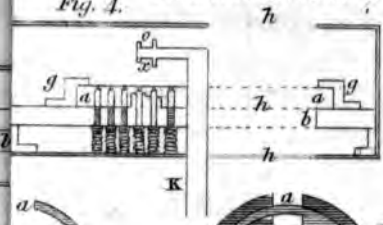
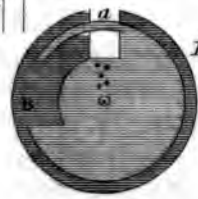


Fig. 5

Fig. 8.



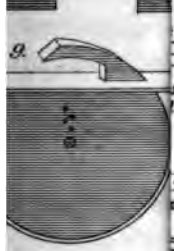


Fig. 18.

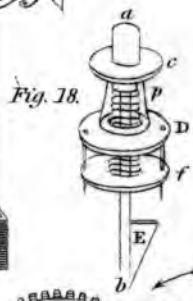
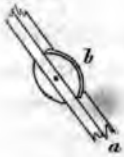


Fig. 13.



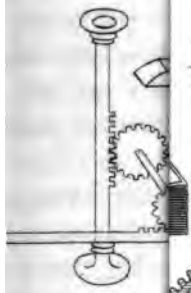


Fig. 21.

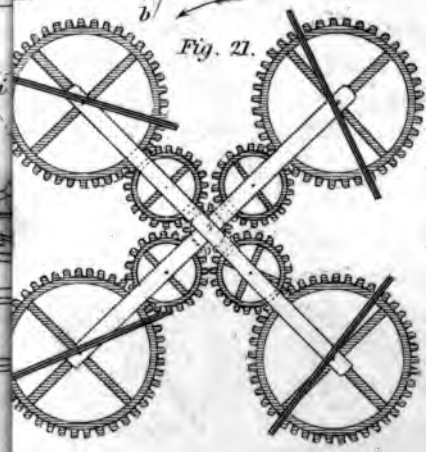
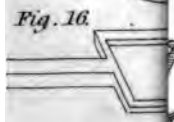


Fig. 16.



shewn in Fig. 18. *e*, wedge (or roller, as Fig. 13). *g*, roller, to answer as *e*, Fig. 15. *h*, a circular rim or button turning through a hole in the lock with a piece attached to it to stop the descent of pusher.

Fig. 17, view of the button as it appears out of the lock. A, the raised part, which passes the lock, making a shoulder below on *g*, which moves in the room, and has a small pin *f* to turn the stopper *e*, under the pin *d*, in the pusher by the motion of the button. B, slit to permit the pin *d* to pass when the catch is used. *c*, slit to let the end or bottom part of wedge *i* enter in descending to release the catch *e* stopper. *h*, notch in the raised part of button within the lock to limit its motion by a check pin, against which it stops.

Fig. 18, *a b*, pusher passing through hollow handle *c*, the part *p* of the pusher being made with a shoulder to take upon the spring within the handle, while a plate *f* with the size of the pusher retains the spring, by being screwed together with the handle at *d* to the door. The part *a p* of pusher takes off at *p* to admit plate *f*.

Fig. 19, view of a lock, with the catch-frame moving through a similar frame of the key-bolt. The key enters through this frame as shewn in Fig. 16, and turns the moveable plate *m*, which having teeth taking upon the pins through the key-bolt or frame at *a*, turns it, as in Fig. 20. *b b*, frame supporting fixed plate on which the moveable plate *m* turns, and under which the springs to the pins or nails are fixed. *s s*, screws to fasten the lock to the door when the works are let into it, so as to make the back of the lock *w x y z* even with the door.

Additional

Additional Experiments and Remarks on an artificial Substance, which possesses the principal characteristic Properties of Tannin.

By CHARLES HATCHETT, *Esq. F.R.S.*

From the PHILOSOPHICAL TRANSACTIONS of the ROYAL SOCIETY.

§ I.

WHEN I had ascertained that carbonaceous substances, whether vegetable, animal, or mineral, were capable of being converted into a product, which, by its effects on earthy and metallic solutions, on dissolved gelatine, and on skin, resembled the natural vegetable principle called tannin, I was at first inclined to give it the name of artificial or factitious tannin; but some eminent chemists of this country, for whose opinions I have the highest respect, considered this name as objectionable; for although the artificial substance resembles tannin in the particulars above stated, yet in one character there appears to be a very considerable difference, namely, the effect of nitric acid; for by this the artificial substance is produced, whilst the varieties of natural tannin are destroyed. Such an objection, sanctioned by such authority, induced me to alter the title of my paper, and to expunge the word *tannin* wherever it had been applied to the artificial product.

In order to satisfy myself more fully on this point, I have, since the communication of my former paper, made a few experiments on the comparative effects produced by nitric acid on those substances which contain the most notable quantities of tannin, and of these I shall now give a succinct account, and shall also cursorily notice other experiments,

experiments, in which a tanning substance has been produced, under circumstances different, in some measure, from those which have been already described.

§ II.

Although I cannot as yet assert, that the artificial tanning product is absolutely indestructible when repeatedly distilled with different portions of nitric acid, yet the following experiments will prove, that the destructibility of it by this method must at least be a work of considerable time and difficulty.

1. Twenty grains of this substance were dissolved in half an ounce of strong nitric acid, the specific gravity of which was 1.40. The solution was then subjected to distillation until the whole of the acid had come over; after which it was poured back upon the residuum, and the distillation was thus repeated three times.

Care was taken not to over-heat the residuum; and this, when examined, did not appear to have suffered alteration in any of its properties.

2. Ten grains of the artificial tanning substance, mixed with ten grains of white sugar, were dissolved in half an ounce of nitric acid, and the whole was distilled to dryness.

The residuum being then dissolved in boiling distilled water, and examined by solution of gelatine and other reagents, was found to be unchanged in every respect.

3. This resembled the former, only that gum arabic was employed in the place of sugar. The result was the same.

4. A quantity of dissolved isinglass was precipitated by a solution of the artificial tanning substance, and the precipitate having been well washed with hot distilled water, was afterwards gradually dried. It was then digested in strong

strong nitric acid, which after some time acted powerfully upon it; much nitrous gas evolved, and a dark brown solution was formed. This was evaporated to dryness; and after having been completely dissolved in boiling distilled water, was examined by nitrate of lime, acetite of lead, muriate of tin, and solution of isinglass, all of which formed copious precipitates, similar in every respect to those produced by the artificial tanning substance, which had not been subjected to the above-described process.

5. A portion of the precipitate, formed by isinglass and the tanning substance, was dissolved in pure muriatic acid, and was afterwards evaporated to dryness. Boiling distilled water dissolved only a small part, and the solution, which was of a dark beer colour, did not precipitate gelatine, although it acted upon muriate of tin, and sulphate of iron; for with the former it produced an ash-coloured precipitate, and with the latter a slight deposit of a reddish-brown colour.

6. As so small a part of the precipitated isinglass had been thus rendered soluble in boiling water, the residuum was treated with nitric acid, as in experiment 4, after which, being evaporated to dryness, it was found to be completely soluble in water, and precipitated gelatine as copiously as at first.

7. I dissolved twenty grains of the pure tanning substance in about half an ounce of muriatic acid; but, after distillation to dryness, the residuum in every respect appeared to be unchanged.

In addition to the above experiments may be added, that the solutions of the artificial tanning substance seem to be completely imputrescible, neither do they ever become mouldy like the infusions of galls, sumach, catechu, &c.

Having

Having thus ascertained the very unchangeable nature of this substance, I made the following comparative experiments on galls, sumach, Pegu cutch, kascutti, common cutch, and oak bark.

8. Twenty grains of powdered galls were dissolved in half an ounce of the strong nitric acid; the solution was then evaporated to dryness, and the residuum dissolved in boiling water. This did not produce the smallest effect on dissolved gelatine.

9. A strong infusion of galls evaporated to dryness, and treated as above, was totally deprived of the tanning property.

10. Isinglass precipitated by the infusion of galls, was dissolved in nitric acid, and examined as in experiment 4, but no trace of tannin could be discovered.

11. Twenty grains of sumach were dissolved in half an ounce of the strong nitric acid, and treated as in experiment 8, after which it appeared that the tannin was destroyed.

12. Twenty grains of Pegu cutch (which contains a considerable quantity of mucilage) were subjected to a similar process, by which much oxalic acid was obtained, but every vestige of tannin was obliterated.

13. Twenty grains of the catechu called Kascutti afforded results similar to the above.

14. Twenty grains of the common cutch or catechu being dissolved in nitric acid, evaporated to dryness, dissolved in water, and examined by solution of isinglass, rendered the latter turbid, a tenacious film was deposited, which was insoluble in boiling water, and was evidently composed of gelatine and tannin.

15. Twenty grains of prepared oak bark, by the like treatment, afforded a solution in water, which still acted in some measure upon gelatine, as it caused a solution

of isinglass to become slightly turbid, and a film completely insoluble in boiling water was, as in experiment 14, deposited on the sides and bottom of the vessel.

16. Infusions were prepared as nearly as possible of equal strength from galls, sumach, shavings of oak-wood, oak-bark, and the artificial tanning substance; half an ounce in measure of each was then put into separate glasses, and one drachm in measure of the strong nitric acid was added.

The different infusions were then examined by solution of isinglass, and I found that those of galls, sumach, and oak-wood, were not rendered turbid, whilst the contrary happened to the infusions of oak-bark, and of the artificial substance; for these continued to precipitate gelatine until four drachms, or half an ounce, of the nitric acid had been added to each half ounce of the infusion.

When the results of these experiments are compared, they seem to establish, that although the artificial product is by much the most indestructible of all the tanning substances, yet there is some difference in this respect even between the varieties of natural tannin; and that common catechu, and the tannin of oak-bark, resist the effects of nitric acid much longer than galls, sumach, kascutti, and Pegu cutch. The last, as I have observed, is replete with mucilage, and by nitric acid yields a large quantity of oxalic acid; it also appears to be the most destructible of all the varieties of catechu, and on this account I attempted, although without success, to promote the destruction of the properties of the artificial substance, by adding gum arabic in one case, and sugar in another, to different portions, previous to exposing it to the action of the nitric acid. I am, however, convinced, that the presence of gum or mucilage in natural substances which contain tannin, renders this more speedily

speedily destructive of nitric acid; and I shall soon have occasion to notice some experiments which tend to prove, that the presence of gum or mucilage in certain bodies, also prevents or impedes more or less the formation of the artificial tanning substance. The cause of this difference I am inclined to suspect is, that in those bodies the gum or mucilage is not simply mixed, but is present in a state of chemical combination; by which certain modifications, produced by the action of nitric acid upon the elementary principles of the original substance, become facilitated.

§ III.

A. When sulphuric acid was added to a solution of the artificial tanning substance, the latter became turbid, and a copious brown precipitate subsided, which was soluble in boiling distilled water, and then was capable of precipitating gelatine.

B. The same effect was produced by muriatic acid; so that in these particulars the artificial tanning substance was found to resemble precisely the *tannin* of galls, and of other natural substances *.

C. Carbonate of potash, when added to a solution of the artificial tanning substance, deepens the colour, after which the solution becomes turbid, and deposits a brown magma.

D. Five grains of the dry substance were dissolved in half an ounce of strong ammonia; the whole was then evaporated to dryness, and, being dissolved in water, was found not to precipitate gelatine unless a small portion of muriatic acid was previously added.

* Mr. Davy on the Constituent Parts of Astringent Vegetables. Phil. Trans. 1803, p. 240, 241.

E. Another portion of the same substance which had been dissolved in ammonia was evaporated in a long-necked matrass, and was kept in very hot sand during half an hour; at first some ammonia arose, and afterwards a yellow liquor, which had the odour of burned horn. The residuum was then examined, and was found to be nearly insoluble in water, to which it only communicated a slight yellow tinge.

F. It is remarkable, that the dry artificial tanning substance, although prepared from vegetable matter, should, when placed on a hot iron, emit an odour very analogous to burned animal substances, such as horn, feathers, &c.; this I found also to be the case in the experiment which has been related, and I was desirous therefore to ascertain more accurately the effects of heat on this substance when distilled in close vessels.

I took some very pure vegetable charcoal, which had been exposed to a red heat in a retort for more than an hour, and by nitric acid converted it into the artificial tanning substance.

Twenty grains of this, rendered as dry as possible, were put into a small glass retort, to which a proper apparatus terminating in a jar filled with quicksilver, and inverted in a mercurial trough was adapted. The retort was placed in a small furnace, and was gradually heated by a charcoal fire until the bulb became red hot.

When the retort became warm, and after the expulsion of the atmospheric air, a very small portion of water arose, which settled like dew on the sides of the vessels; this was succeeded by a little nitric acid, from which the tanning substance had not been completely freed, and soon after a yellowish liquor came over, which was in so very small a quantity as only to stain the upper part of the neck of the retort: as nothing more seemed to be produced,

produced, I then raised the fire, when suddenly the vessels were filled with a white cloud, and so great a portion of gas was almost explosively produced as to overset the jar; this gas, by its odour, appeared to be ammonia, which in the first instance had formed the white cloud, by combining with the vapour of the nitric acid with which the vessels were previously filled *. Another jar was speedily placed in the room of that which had been overturned, and a quantity of gas was slowly collected; this proved to be carbonic acid, excepting a very small part, which was not taken up by solution of caustic potash, and which, as far as the smallness of the quantity would permit to be determined, appeared to be nitrogen gas. There remained in the retort a very bulky coal, which weighed eight grains and a half; this by incineration yielded one grain and a half of brownish-white ashes, which consisted principally of lime, but whether any alkali was also present I cannot positively assert, as the trace which I thought I discovered of it was very slight.

I shall for the present postpone any remarks upon this experiment, as I wish to proceed in the account of others which have been made on the artificial tanning substance.

G. Fifty grains of this substance were dissolved in four ounces of water, and were afterwards precipitated by dissolved isinglass, eighty-one grains of which became thus combined with forty-six grains of the tanning substance.

The remaining portion of the latter was not precipitated, and was therefore separated by a filter, and evaporated to dryness. It then appeared in the state of a light brittle substance, of a pale cinnamon-brown colour;

* After the experiment the receiver was found to be thinly coated with a white saline crust.

and

and it is very singular, that although charcoal is an inodorous body, and although the artificial tanning substance, when properly prepared, is likewise devoid of smell, (unless a certain pungent sensation which may be perceived upon first opening a bottle containing the powder after agitation should be so termed, but which seems rather to be a mechanical effect,) yet this substance possessed a strong odour not very unlike prepared oak-bark, and this odour became much more perceptible when the substance was put into water, in which it immediately dissolved. The solution was extremely bitter, and acted but slightly on dissolved isinglass, with which however it formed some flocculi; with sulphate of iron it produced a brown precipitate; with muriate of tin one which was blackish-brown; nitrate of lime had not any effect; but acetite of lead occasioned a very copious precipitate, of a pale brown colour. This substance therefore appeared to be a portion of the tanning matter, so modified as partly to possess the characters of extract *.

Other experiments were made on the tanning substance prepared from various bodies, which by the dry and by the humid way had been previously reduced to the state of coal; but these I shall here omit, and shall pass to the description of a series of experiments, by which I obtained a variety of the artificial tanning substance in a way different from that which has been related, and with which I was unacquainted when my former paper was written.

* When added to a solution of carbonate of ammonia, it produced some effervescence, but its peculiar vegetable odour did not suffer any diminution.

§ IV.

§ IV.

I made several unsuccessful attempts to form the artificial substance by means of oxymuriatic acid, and it therefore appeared certain, that although a variety of the tanning matter could be produced by the action of sulphuric acid on resinous substances, yet the most effective agent was nitric acid, which readily formed it when applied to any sort of coal.

But I nevertheless suspected, that possibly this substance, or something similar to it, might be produced without absolutely converting vegetable bodies into coal; for it seemed, as I have observed in my former paper, that this only served to separate the carbon in a great measure from the other elementary principles (excepting oxygen) which were combined with it in the original substance, and thus to expose it more completely to the effects of the nitric acid, as well as to prevent the formation of the various acid products, which are so constantly afforded by the organized substances when thus treated. At first I had some thoughts of employing touchwood in this experiment, but not being able immediately to procure any, it occurred to me, that indigo might probably answer the purpose; for from some experiments made by myself, as well as from those described by Bergman *, I well knew that the proportion of carbon in this substance is very considerable. The following experiment was therefore made.

1. On one hundred grains of fine indigo which had been put into a long matrass, one ounce of nitric acid diluted with an equal quantity of water was poured, and, as the action of the acid was, almost immediately, ex-

* Analysis Chemica Pigmenti Indici. Opuscula Berg. tom. V. p. 36.

tremely

tremely violent, another ounce of water was added. When the effervescence had nearly subsided, the vessel was placed in a sand-bath during several days, until the whole of the liquid was evaporated.

On the residuum, which was of a deep orange colour, three ounces of boiling distilled water were poured, by which a considerable part was dissolved.

The colour of the solution was a most beautiful deep yellow, and the bitter flavour of it surpassed in intensity that of any substance in my recollection; it was examined by the following re-agents.

Sulphate of iron produced a slight pale yellow precipitate.

Nitrate of lime only rendered it a little turbid; after which a small portion of white powder subsided, which had the characters of oxalate of lime.

Muriate of tin produced a copious white precipitate, which afterwards changed to a yellowish-brown.

Acetite of lead formed a very beautiful deep lemon-coloured precipitate, which possibly may prove useful as a pigment.

Ammonia rendered the colour much deeper; after which the liquor became turbid, and a large quantity of fine yellow spiculated crystals were deposited, which being dissolved in water, did not precipitate lime from its solutions.

The flavour of these crystals was very bitter, and I suspect them to be composed of ammonia combined with the bitter principle first noticed by Welther *.

Lastly, when dissolved isinglass was added to the yellow solution of indigo, it immediately became very turbid, and a bright yellow substance was gradually de-

* Thomson's System of Chemistry, 2d edit. vol. IV. p. 246.

posited

posited, and coated the sides of the glass jar with a tough elastic film, which was insoluble in boiling water, and possessed the characters of gelatine combined with tanning matter.

By this experiment I therefore ascertained, that a variety of the artificial tanning substance could be formed without previously converting the vegetable body into coal; and I have since discovered, that although indigo more readily yields this substance than most of the other vegetable bodies, yet, in fact, very few of these can be regarded as exceptions, when subjected to repeated digestion and distillation with nitric acid.

2.—A. In my former paper I have stated, that common resin, when treated with nitric acid, yielded a pale yellow solution with water, which did not precipitate gelatine, and that it was requisite to develope part of the carbon in the state of coal by sulphuric acid, before any of the tanning substance could be produced; but having again made some of these experiments, I repeated the abstraction of nitric acid several times, and then observed, that the solution of resin in water acted upon gelatine similar to the solution of indigo which has been described, and formed a tough yellow precipitate, which was insoluble in boiling water.

With other re-agents the effects were as follows.

Sulphate of iron, after twelve hours, formed a slight yellow precipitate.

Nitrate of lime did not produce any effect.

Muriate of tin, after twelve hours, afforded a pale brown precipitate.

And acetite of lead immediately formed a very abundant precipitate of a yellowish-white colour.

I repeated this experiment on common resin, and remarked, that during each distillation nitrous gas was

produced, whilst the strength of the acid which came over was diminished; the cause therefore of the change in the properties of the resin seemed to me very evident, and I was induced to extend the experiments to various resinous and other substances; but as the process was uniformly the same, I shall only notice the principal effects.

B. Stick lac, when separated from the twigs, and treated as above described, copiously precipitated gelatine.

C. Balsam of Peru during the process afforded some benzoic acid, and gelatine was precipitated by the aqueous solution.

D. Benzoin also, after the sublimation of some benzoic acid, yielded a residuum, which with water formed a pale yellow solution, of a very bitter flavour.

This solution with sulphate of iron afforded a slight pale yellow precipitate.

With nitrate of lime not any effect was produced.

The solution with muriate of tin became turbid, and a small quantity of brownish-white precipitate subsided.

Acetite of lead immediately produced a copious pale yellow precipitate.

And solution of isinglass formed a dense yellow precipitate, which was insoluble in boiling water.

E. Balsam of Tolu, like balsam of Peru and Benzoin, afforded some benzoic acid; and the residuum being dissolved in water, was found to precipitate gelatine.

F. As the results of the experiments on dragon's blood were somewhat remarkable, I shall here more particularly relate them. One hundred grains of pure dragon's blood, reduced to powder, were digested in a long matrass with one ounce of strong nitric acid; the colour immediately changed to deep yellow, much nitrous gas was evolved,

and

and to abate the effervescence, one ounce of water was added. The digestion was continued until a deep yellow dry mass remained, and the matrass being still kept in the sand-bath, a brilliant feather-like sublimate arose, which weighed rather more than six grains, and had the aspect, odour, and properties, of benzoic acid *.

The residuum was of a brown colour, and with water formed a golden yellow-coloured solution, which by nitrate of lime was not affected.

With sulphate of iron it afforded a brownish-yellow precipitate.

With muriate of tin the result was similar.

With acetite of lead a lemon-coloured precipitate was produced.

Gold was precipitated by it in the metallic state, whilst the glass vessel acquired a tinge of purple:

And dissolved isinglass produced a deep yellow deposit, which was insoluble in boiling water.

A portion of the same dragon's blood was simply exposed to heat in the same matrass, but not any appearance of benzoic acid could be discovered. I am therefore induced to believe, that in the first experiment it was obtained as a product, and not as an educt, a fact which as yet has not been suspected.

G. Gum ammoniac afforded a brownish-yellow solution, the flavour of which was very bitter and astringent.

* According to these experiments, dragon's blood ought to be arranged with benzoin and the balsams, but as the samples of this drug are not always precisely similar, it would be proper to examine every variety. That which was employed in my experiments was a porous mass, of a dark red, and was sent to me by Messrs. Allen and Howard, of Plough-court, in Lombard-street.

By sulphate of iron this solution only became of a darker colour, but did not form any precipitate.

Nitrate of lime rendered it turbid, and produced a slight precipitate.

Muriate of tin formed a copious yellow precipitate.

Acetite of lead produced a similar effect:

And gelatine yielded a bright yellow deposit, which was completely insoluble by boiling water.

H. Asa fœtida yielded a solution which also precipitated gelatine like the substances above described.

I. Solutions of elemi, tacamahac, olibanum, sandarach, copaiba, mastich, myrrh, gamboge, and caoutchouc, were next examined, but these, although they precipitated the metallic solutions, did not affect gelatine. It is possible, however, that they might have produced this effect, had they been subjected to a greater number of repetitions of the process.

K. Sarcocol, in its natural state, as well as the gum separated from it by water, when treated with nitric acid, did not precipitate gelatine; but produced effects on the metallic solutions similar to the above-mentioned substances.

L. Gum arabic afforded oxalic acid, but not any of the tanning matter.

M. Tragacanth yielded an abundance of sacfactic acid, of oxalic, and of malic acid, but not the smallest vestige of the artificial tanning substance.

N. Manna, when treated with nitric acid in the way above described, afforded oxalic acid, part of which was sublimed in the neck of the vessel.

The residuum with water formed a brown solution, which yielded a pale yellow precipitate with sulphate of iron.

Muriate

Muriate of tin produced a pale brown precipitate.

Acetite of lead formed one of a brownish-white hue.

Lime was copiously precipitated from the nitrate of lime in the state of oxalate; but not the smallest effect was produced on solution of isinglass.

O. Liquorice, however, afforded a different result; for although the solution after the process with nitric acid resembled in appearance that which was yielded by manna, yet the effects were not the same.

Sulphate of iron, after twelve hours, produced a slight brown precipitate.

Muriate of tin had a similar effect.

Acetite of lead formed a brownish-red deposit.

Nitrate of lime also occasioned a brown precipitate:

And solution of isinglass rendered it very turbid, and produced a yellowish-brown precipitate, which was insoluble in boiling water, and possessed all the other characters of gelatine combined with the tanning substance.

P. Guaiacum, the properties of which are so singular in many respects, afforded results (when treated with nitric acid in the manner which has been described) different from the resins, although its external and general characters seem to indicate that it appertains to those bodies.

Nitric acid acted upon it with great vehemence, and speedily dissolved it. The residuum which was afterwards obtained was also found to be almost totally soluble in water, and the solution acted on the metallic salts like those which have already been noticed, but with gelatine it formed only a very slight precipitate, which was immediately dissolved by boiling water; and the remainder of the solution being evaporated, yielded a very large

large quantity of crystallized oxalic acid; so that in this respect guaiacum was found to resemble the gums, and to be totally unlike the resins *.

* The properties of guaiacum which have been described, as well as those which were previously known, appear to indicate, that it is a peculiar substance of a nature distinct from the resins, balsams, and even the gum resins.

So remarkable indeed is this substance, that an accurate series of experiments on the whole of its properties may justly be placed amongst the chemical desiderata.

TO BE CONCLUDED IN OUR NEXT.

Description of a horizontal Wind-Mill of a new Construction, the Principles of which may be applied to Water-Wheels.

Communicated by the Inventor Mr. JOHN JACKSON.

With an Engraving.

Fig. 21 (Plate XII.) represents a plan or birds-eye view of the apparatus.

The various wheels requisite to give the proper positions to the vanes which are placed under the lower arms of the wheel, for sake of distinction, may be denominated the centre, the mean, and extreme wheels. Each extreme wheel has twice as many teeth as the centre, and is consequently twice as much in circumference. The mean wheel may be of any size, or consist of any number of teeth, according to the length of the arm or the diameters of the other two. The mean and extreme wheels are attached to the arms by their axles, and are carried about with it when in motion. The extreme wheel is fixed to its axle, which passes through the extremities

tremities of the lower and upper arms. The vane is fixed to this axle, and consequently turns with it.

The centre wheel consists of a drum or hollow shaft, through which the axle of the wheel passes. A handle is fixed to the lower extremity of the drum-shaft, by which the centre wheel is occasionally moved: this arm serves also as an index, and communicates with a circle, representing the horizon, on which are delineated the points of the compass. When the machine is in motion, the index is set and fastened to that point from which the wind blows, in order properly to rectify the vanes to the wind, or place them in the most advantageous position. Any motion of the index, to the right or left of this point, will alter the position of the vanes, and occasion a diminution of power in the machine; and it is by this index that the machine is stopped or made to turn in a contrary direction, a peculiarity, when referred to wind-mills, that exclusively belongs to this contrivance.

The position of the vanes to each other, and to the wind, may be easily conceived by inspecting the figure. The nearest vane on the left hand has its plane in the direction of the arms between which it is placed, and is directly exposed to the wind, represented by an arrow. The opposite vane, or farthest on the right hand, has its plane at right angles to the arms, is represented edgewise to the wind, and consequently little acted upon by it. Each of the other two vanes make an angle of forty-five degrees with the arms, but their planes are in contrary directions; the wind acts obliquely upon them, and with only half the force of a direct position. As the whole turns round, each vane gradually and successively assumes the positions above described. In one revolution each extreme wheel turns half round each vane, therefore presents its sides alternately to the wind.

The

The power of this wheel is as the length of the arm and magnitude of the vane. The dimensions of each may be varied according to the degree of power or velocity required in the machinery, or purposes to which the wheel may be applied.

This explanation of the general plan may be thought sufficient. A minute detail of every part would be trespassing on the province of the mechanic, and be uninteresting to the general reader.

Some of the properties peculiar to this contrivance are: 1. That it is equally adapted to turn an axle in any position, and in either element. 2. The facility with which it may be set to the wind, have its power regulated, stopt, or made to turn in a contrary direction. 3. With a vertical axle it will not require a building of any particular shape or height.

Memoir on a Discovery of factitious Pozzolana, which is not the Product of any volcanic Substance, and which nevertheless possesses all the good Qualities of the Italian Pozzolana, without its Defects. By M. DODUN.

From the JOURNAL DE PHYSIQUE.

THE discovery which I have the honour to submit to the attention of the Institute has been authenticated already for seventeen years past.

The history of this discovery, its nature, its use, its solidity, its advantages to the French Empire, and its profitable employment in public and private works, shall form the subjects of this memoir.

Use has long been made in Flanders, and the adjacent countries, of the remains of old volcanic substances known by the names of *trass* and *Tournay ashes*, as substitutes for

for the pozzolana of Italy. Not long since a distinguished mineralogist, M. Faujas, has proved, by conclusive experiments made by order of the former government of France, that some products of the eruptions of old volcanoes in the *ci-devant* Vivarais, have the qualities of the Italian pozzolana, and may be used as substitutes for it.

It is well known that M. Baggé, a Swede, has succeeded by his researches, with the aid perhaps of chance, in composing a cement of artificial pozzolana with a black, hard, and slaty schistus.

But till in the year 1787 no one had yet supposed that the French territory contained an abundance of non-volcanic substances, capable of being substituted with equal economy and advantage in the place of the Italian pozzolana.

The habit of sounding rocks, and their consecutive examination, which always give the lithologist the advantage of judging of their nature by the first view, could not fail, in a region so interesting as that of the black mountain in the vicinity of Castelnaudary, to fix my attention upon an immense quantity of tubercular masses of calciform iron ore, the beds of which, eight, nine, and ten feet in depth, rigorously follow the direction of slightly-inclined planes.

Hitherto this observation had presented nothing very interesting. The lithologist saw in these tertiary *detritus* only oxyds of iron, conglomerated indeed with fragments of fine angular quartz, which led him to suppose that these oxyds proceeded from some hematite ores situated in the vicinity, which water, *the chief agent of the vitality of the globe, alternately creating and destroying, had reproduced and re-elaborated in its last operations.*

But the view of several substances of the same nature, and of violet, brown, and black colours, diffused over

the surface of the intermediate strata, which perfectly resembled some compact lavas in colour, could not escape his attention in a country where there is no apparent vestige of antient craters, and no product of volcanic eruptions.

It was, however, evident that these stones had experienced the action of fire. This remark excited my curiosity. I soon observed similar ones, which, still piled upon each other, had the appearance of having been arranged by the hand; and I soon discovered that they had served as hearths for the fires which the women and children who attend the cattle are in the habit of kindling during the winter season. I also saw stones of the same kind used to support and inclose the earths which are calcined for the purpose of employing their ashes as manure. Thus I obtained a clue to the subject of my inquiry.

Their resemblance to volcanic products suggested to me the idea of attempting to compose a cement by treating them after the manner of Pozzolanas. The great quantity of iron which these oxyds appeared to contain, the abundance of siliceous particles inclosed in them, the alumine which evidently formed an integrant part in their composition, their great weight, their not effervescing with the acids; all these circumstances led me then to imagine, that a cement composed of them would acquire consistence in water. Nor was I deceived in my expectations.

A series of experiments, prosecuted for the space of fifteen months, for the purpose of ascertaining, in an invariable manner, the proportions of lime which these oxyds could absorb, in order to harden in water, and not crack in the open air, convinced me that my factitious

pozzolana

pozzolana had all the properties of the Italian pozzolana without being subject to its defects.

From this moment I determined to propose its use in public works. I requested, in order well to ascertain its merits, that experiments might be made in presence of the commissioners of the *ci-devant* province of Languedoc, and of the directors of the canal of Languedoc. I also requested that comparative trials might be made both of my pozzolana and of the Italian. Large blocks were accordingly made of both pozzolanas, and immersed in the reservoir of Saint Roch, at Castelnaudary. Coatings were also made of it; and when six months after the water was withdrawn from them, it was found that the factitious pozzolana had acquired a solidity at least equal to that of Italy. The coatings made with the Italian pozzolana were cracked and broken. Those of the factitious pozzolana had preserved the unity of their surface.

My researches on the amelioration of our cements, and especially on the nature of the substances adapted for the formation of factitious pozzolanas, have led me to try the calcination of numerous schistuses, bituminous, ferruginous and argillaceous. The black slaty schistus of M. Baggé, which is very common in France, was not neglected. It is nearly the same as that which M. Gratien, the father, tried at Cherbourg last year; but I have constantly found that these schistuses contain too little iron. I perceived that the hardening operation of water upon them was slow and feeble, and that they owed their solidification in it solely to the good quality of the lime which is then their strongest cement.

I was thus obliged to return to my quartziferous oxyds of iron, as containing a larger quantity of ferrugineous principles. I repeat, with the learned Faujas, that it is solely to the ferrugineous parts contained in the pozzo-

lanas that they owe their property of hardening in water. Of this truth I could adduce many striking proofs.

Numerous experiments have demonstrated to me, that the pozzolana which acquires solidity the soonest, and hardens the most in water, is not fit to be employed in the open air, but splits and cracks in every direction when exposed to it. However studied and varied my combinations may have been, I have never been able to employ with success the same pozzolana in water and in the open air. That which is adapted for the air, and which acquires and preserves its tenacity in it, assumes but a very imperfect solidity in water. This difficulty, the cause of which will be evident to the Institute, has obliged me to keep two qualities of factitious pozzolana. A paper of instructions concerning their respective uses accompanies those which are sold, and their colour alone is sufficient to distinguish them.

The factitious pozzolana adapted for works under water is of a reddish-brown colour. That which is appropriated to works exposed to the air is of a violet-black. The latter is used for terraces, the coverings of basins, or light roofings. Bridges might be made of it in a single cast. I have seen it adhere so firmly to glazed tiles, that it was necessary to break the tiles in order to detach it from them.

The factitious pozzolana adapted for constructions under water forms in it the most solid substance. In the space of three months it becomes a stone that admits of being polished. The lime is always converted into carbonate of lime in the space of two months and a half.

I generally employ the lime in an impalpable powder, slaked after the manner of Lafaye, for works exposed to the air. Sometimes I use lime in powder for the same purpose. This difference depends upon the degrees of goodness

goodness which the lime may possess upon its being more or less fat, more or less poor. Practice enables us to distinguish them by the mere aspect.

The use of lime in powder has always appeared to me to merit attention in the fabrication of mortars or cements. I amalgamate my factitious pozzolana with it in determinate quantities as soon as I know the proportions of the lime. After having made the mixture, I deposit it in a sack. Thus the composition is not left to workmen destitute of scientific principles; but regulating the proportions of the different ingredients myself, I am always assured of the solidity of my cements.

I shall now describe the external characters of the quartziferous ferrugineous oxyds which form the base of my factitious pozzolana, together with the analysis which I made of them about eighteen years ago. I shall content myself with presenting their results both by the humid and the dry process, compared with those of the Italian pozzolana.

External Characters of the quartziferous Oxyds of Iron.

Their colour previous to calcination is a reddish-brown, or violet. A slight torrefaction gives them either a lighter red or a deep violet-red tinge; which by a stronger torrefaction is converted either into an intense brown or a violet-brown, inclining to a black. The degree of calcination for use is limited to these two torrefactions.

Urged longer by the fire, the colour becomes a deep black; the substance then becomes porous, and perfectly similar to some lavas of antient or modern volcanoes, with which it may then be easily confounded.

The fracture is granulated, and a little earthy. We may distinguish in it by the naked eye small crystals of quartz, and almost always angular fragments of grey or milk-

milk-coloured quartz. By a magnifying glass, of strong powers, we may distinguish in some specimens needles of schorl, the amphibole of Haüy, and sometimes small tourmalines.

When breathed upon, their smell is strongly argillaceous.

They do not strike fire with steel, unless the metal comes in contact with some quartzose particles.

They do not effervesce with acids either hot or cold.

The magnet acts a little upon these oxyds before calcination, and strongly or sensibly after it.

The average weight of the cubic foot is 125°; that of the Italian pozzolana is only 91°.

Analysis by the humid Process.

I shall not fatigue the Assembly with a detail of the manipulations required for the decomposition of these substances. I shall merely say, that silex, iron, alumine, and a small portion of manganese, are the constituent parts of these oxyds.

I have several times repeated these experiments upon various specimens, and obtained for the average result in the docimastic quintal, 50 parts silex, 31 iron, 16 alumine, 3 manganese, or loss.

If we compare this analysis with that of the pozzolana of Italy, which contains in the hundred 51 silex, 25 alumine, 16 iron, 3 lime, and 6 loss, we shall be able to appreciate their respective properties according to the proportions of their constituent parts. It is on account of the excess of alumine that the pozzolanas of Italy, when employed for coatings, crack and split in the open air. This defect proceeds from their great oxydation. I have succeeded in restoring to it the principles which its decompositions have caused it to lose.

Analysis

Analysis by the dry Process.

I have endeavoured to obtain a regulus from these oxyds of iron, by the application of violent fire. I have followed the processes of Kirwan for the fusion of the siliceous and argillaceous iron ores, but have never been able to obtain a metallic button; I have found at the bottom of the crucible only an opaque black vitrified mass, or a scoria in the state of pig-iron.

Curious to know whether, by exposing them to the action of the blow-pipe, and using charcoal for the cupel, and borax for the flux, I could obtain a malleable button, I found that I could form only a cavernous ingot, brittle both hot and cold.

Placed upon the glass cupel, according to my method described in the Journal de Physique, tom. XXXI, this oxyd is fused at the second application. The cupel assumes a green colour. We see the small grains of iron successively pass from a black to a dark-green colour, then to a light green, and finally disappear by evaporation. There remains upon the globule only a slight greenish black tinge.

From all these facts it appears to result, that this oxyd is entirely deprived of its metallic principle, and that its super-oxydation renders it irreducible and refractory.

Art may also avail itself of these oxyds for the purpose of house-painting. After repeated washings, I have extracted from it a beautiful red-brown pigment, equal to that of commerce, which I have employed with good success.

On Niccolanum, a Metal lately discovered, which in many Respects resembles Nickel. By Dr. RICHTER.

From the ANNALES DE CHIMIE.

1. I HAVE several years ago conjectured, in analyzing the cobalt ores of Saxony, that they contained, besides cobalt, arsenic, copper, nickel, and iron, another metal which resembled nickel in many of its properties; but the means I have hitherto employed in order to separate it, did not afford me any satisfactory results.

I was chiefly surprised that nickel, after having been purified by the liquid process from cobalt, arsenic, and iron, and then reduced without the addition of a combustible body, never formed a mass, but was always dispersed in the form of granulations in a hard heavy mass, which had the appearance of a substance remaining from vitrified copper.

This hard substance had no metallic lustre, neither was it attracted by the magnet. Its colour was a blackish-grey on the surface, with a small degree of brilliancy; when pulverized, it was brown, greyish, and greenish.

2. Some weeks ago I exposed to reduction, *per se*, nearly half a pound of oxyd of nickel, which I had purified as well as possible by the liquid process, for the space of more than a year, and at a considerable expense. As this oxyd was not of a lively green, I thought this was caused by the extractive matter contained in the potash employed for the precipitation of the sulphate of nickel from ammoniac and potash: it is true, that this triple combination had not the beautiful grass-green colour which it commonly possesses; but I thought that this might proceed from the substitution of the potash to the ammoniac

ammoniac and copper mixed, which could not be separated but by reduction *per se*.

From these ideas I hoped to have at least four ounces of perfectly-pure nickel, but was much disappointed to find in the crucibles, which were deformed in the usual manner, and perforated by the vitrified copper, a rugged mass, which had the appearance I have before mentioned, and which contained only a morsel of about two drachms and a half, and consequently in the two crucibles only five drachms of pure nickel. I reduced to powder, in an iron mortar, the mass, which cannot properly be called scoria, and separated from it, by the sieve and the magnet, the particles of nickel which it might still contain, which yielded near two drachms and a half more; and, that nothing might be lost, I treated the powder with nitric acid, which attacked it powerfully at first, and gave a solution of nickel, but after that did not exert the least action upon it, so that the powder was but very little diminished in weight: when this matter was exposed to reduction, *per se*, it produced no regulus, but merely became agglutinated in its parts.

3. Having again pulverized the mass, which weighed nearly four ounces and a half, I mixed it with one ounce of powdered charcoal, and exposed it to the fire of a porcellain furnace during eighteen hours, in a crucible closed with a luted cover, in a part of the furnace where the heat seemed to me to be most intense. After having broken the crucible, which was in very good condition, I found in it, under a scoria of a deep blackish-brown colour, a well-fused button of metal, which weighed two ounces and three-quarters; it was not perfectly smooth at the part adjoining the scoria, and it had at its inferior part a peculiar shape that was given to it by concavities which were not produced by the crucible.

4. This metal had the grey colour of steel, inclining a little to red; its fracture presented a grain not very fine; it was of considerable hardness; could be extended a little under the hammer in a cold state; heated to redness it bore few strokes of the hammer; it was attracted by the magnet, but not so strongly as either iron or nickel; it had besides several properties in common with nickel, but was totally different from it in others. As several of these properties were such that those who are not well acquainted with nickel in its perfectly-pure state, might easily take it for that metal, I have called it *niccolanum.*

5. The niccolanum was free from all the metals which are found in the cobalt ores, a little copper excepted.

6. The specific gravity of cast niccolanum, which fuses more readily than nickel, is 8,55; and that of forged niccolanum 8,60. When put into nitric acid, and heated, it is attacked much more quickly than nickel. I remember having observed an equally violent action of nitric acid on nickel reduced by charcoal, which I considered as pure, and which I dissolved in order to precipitate from it by potash an oxyd which I might reduce *per se.*

7. The solution of the niccolanum went on very well; when arrived at the point of saturation, it had a blackish-green colour, and it assumed a gelatinous consistence.

8. I employed my first care to separate from it a portion of iron, which I thought it contained, and I let some of it dry a little over a spirit-lamp; the mass became continually of a deeper green, and as it approached to dryness it exhaled much red vapour, and the residue became of a blackish grey; I added distilled water to it, which dissolved but very little of it, and that which was dissolved was an insignificant quantity of nickel.

9. I poured muriatic acid upon the black powder well washed: a green solution was produced, disengaging a strong odour of oxygenated muriatic acid.

10. The muriatic solution was, as well as the nitric solution, of a deep blackish grass-green colour. Being evaporated to dryness, it gave a reddish mass, which became greenish in a moist air, and which communicated the green colour to water in which it was dissolved.

11. This black oxyd of niccolanum was insoluble in nitric acid and in sulphuric acid; but if, for example, sugar or alcohol was added, the solution took place with facility at the boiling point. The sulphate of niccolanum, when combined with water, is also of a blackish-green; but it assumes a pale red colour when deprived of the water.

12. If carbonate of potash be added to one of the preceding solutions of niccolanum, it produces a precipitate of carbonate of niccolanum of a blue colour, inclining a little to the grey and the green, and of a pale tint: this combination is very light and soft; it dissolves in the acids with a strong effervescence. I remember having had, some years ago, this precipitate of bad colour, and not having examined it, because I considered it as a mixture of iron, nickel, and arsenic, (which last often announced itself by its odour of garlic,) but in the sequel I suspected its nature.

13. If we decompose the solution of niccolanum by caustic potash, a precipitate is formed which resembles the carbonate of chromium in colour; that is to say, it is of a deep greenish-blue, which does not change when it is washed; when dried with a gentle heat it assumes a pale colour, which becomes deeper when it is moistened with water.

14. If any of the above-mentioned solutions of niccolanum is mixed with ammoniac in excess, the liquor assumes a pomegranate-red colour, and remains transparent. This proves that it contains no iron, because the latter is not soluble in ammoniac.

By candle-light it is difficult to distinguish this solution from that of perfectly pure nickel; but by day-light the latter is of an amethyst-red, as I have elsewhere remarked.

15. I shall now compare the properties in which niccolanum resembles nickel or cobalt in part or altogether, and those in which it differs from them.

It resembles cobalt, 1, by its property of surcharging itself as it were with oxygene at the expense of the nitric acid, and thus forming a body which resembles the black oxyd of manganese, with respect to its solubility in the acids; 2, by its property of not being reducible without the intervention of a combustible body.

It differs from cobalt, 1, by the blackish-green colour of its solutions, even when they are perfectly neutralized. It is known that the neutral solutions of cobalt in the sulphuric, nitric, and muriatic acids, are of a crimson-red colour, and that the muriate of cobalt only becomes of a greenish-blue on being deprived of water; whence it happens, that an excess of acid produces this colour, because it combines with the water. With the muriate of niccolanum the case is precisely the reverse: combined with water it is green, (though of a less beautiful tinge than the cobalt without water,) and when deprived of water it becomes reddish. 2. By the colour of its carbonate: that of cobalt is of a beautiful purple, but that of niccolanum is of a blueish-green, inclining to a pale grey. 3. By the colour of its oxyd precipitated without carbonic acid: that of cobalt is of a deep blue, and changes

changes even during washing into a blackish-brown; but the oxyd of niccolanum is of a blueish-green, and its colour does not change.

Niccolanum resembles nickel, 1, by its strong magnetic quality, though this is inferior to that of nickel; 2, by its malleability, though that of nickel is greater; 3, by the deep green colour of its solutions, though this is not so beautiful as that of the solutions of nickel; 4, by the loss of this deep green colour when its neutral combinations are deprived of water; 5, by the colour of its acid solution with an excess of ammoniac, which cannot be well distinguished by candle-light.

But niccolanum differs very distinctly from nickel, 1, because it cannot be reduced without the intervention of a combustible body; 2, because nitric acid attacks and oxydates it more easily. Nickel is not near so easily attacked by the nitric acid if it is not mixed with niccolanum, which has almost always been the case with the magnetic nickel that has been considered to be in a state of purity, and which has not been reduced, *per se*, before my discovery; 3, it differs from nickel by the property No. 1, of those in which it resembles cobalt; 4, by the colour of its combinations with the acids, when deprived of water: this colour in nickel is almost a chamois, and in niccolanum reddish, except in the nitrate of niccolanum, which cannot be deprived of water without decomposing it; 5, by the colour of the precipitates described under Nos. 2 and 3 of the properties in which the niccolanum differs from cobalt, which in those of the nickel is a green entirely different from that of the precipitates of niccolanum, which latter, especially the carbonate, are of a much more agreeable green.

Abstract

Abstract of a Memoir on Uranium. By M. Bucholz.

From Gehlen's Chemical Journal.

SINCE the discovery of uranium, in Pechblende, by M. Klaproth, few chemists have made it their study to furnish us with accurate information respecting this metal.

The author procured uranium of the greatest possible purity, by treating a black fossile which contained uranium with nitric acid; after washing, there remained an insoluble mass, containing red oxyd of iron, silex, and sulphur. The liquor containing the nitrates was evaporated during continual stirring. The residuum was heated to redness, in order to decompose the nitrate of iron contained in it; there remained upon the filtre still some oxyd of iron, and the liquor was then of a greenish-yellow colour, which gave reason to suspect also the presence of copper. The whole was precipitated by a great excess of ammoniac, and the mixture put to digest for twenty-four hours. The supernatant liquor assumed a beautiful blue colour: this was decanted. The residue was washed; it had a fine yellow colour, but it still retained some ammoniac, the smell of which manifested itself on triturating it with potash. This ammoniac was expelled by calcination, and the oxyd of uranium was obtained of a fine greenish-yellow colour. This oxyd might still have contained alumine, if that earth formed a part of the fossile employed: it was separated from it by potash. But what was singular, the oxyd of uranium contained $\frac{1}{100}$ of lime, though ammoniac does not precipitate this earth from its solution in nitric acid; it was carried off by the oxyd of uranium.

As the separation of the lime by an alkaline oxalate is attended with some difficulties, since the oxalate of uranium

nium itself is a salt of little solubility, the author separated the nitrate of lime from the nitrate of uranium by repeated crystallizations, the first remaining in the mother-water. This nitrate of uranium, decomposed by potash, gave the oxyd of uranium which he employed for the reduction.

Oxyd of Uranium reduced.

Messrs. Klaproth and Richter have reduced uranium, with more or less labour, to its metallic state. The former obtained, with great difficulty, at 170 degrees of Wedgwood's thermometer, a button of 8,100 specific weight. (See Klaproth's work on the Chemical Knowledge of Minerals, vol. II.) M. Richter, on the other hand, obtained a better-fused button, by calcining the oxyd of uranium with dried bullock's blood: its specific weight was 6,000, which has led the author to suspect that some phosphuret of uranium was formed.

M. Bucholz, who repeated these processes, made several fruitless experiments in order to effect the reduction. He applied the most intense heat to a globule formed of oxyd of uranium and white wax $\frac{20}{100}$ of fluate of lime, carbonate of lime, and charcoal in powder; but the results were not satisfactory.

At last, the author exposed a mixture of 100 grains of oxyd of uranium and 5 grains of charcoal to a white forging heat, for the space of three hours. After cooling he obtained a solid agglutinated mass, which had lost two-thirds of its volume, and of an iron-grey colour. By the aid of a lens, fine needles of a faint metallic lustre were observed in it. This matter, which was very porous, had absorbed a great quantity of water; its weight was 9,000, which undoubtedly would be increased if the button were more compact and well fused. This metal dissolves

solves in nitric acid with a disengagement of nitrous gas. The author concludes from these experiments, that the oxyd of uranium can be reduced, but he does not prove its fusibility.

A hundred grains of metallic uranium were heated to redness in a crucible: the uranium became incandescent like charcoal; it had augmented by $\frac{5}{175}$ grains in the form of a blackish-grey powder.

Metallic uranium is not attacked by dilute sulphuric acid, but boiling concentrated sulphuric acid acts slightly upon it with a disengagement of sulphurous acid. The muriatic acid has also no great action upon metallic uranium; but it dissolves enough of it for oxygenated muriatic acid to be disengaged when the muriate is evaporated and heated to redness; and there remains a greyish-black oxyd, containing 5 or 6 *per cent.* of oxygene, which may be considered as oxyd at the *minimum*, and which assumes a yellow colour in the air: in short, these two acids act so weakly upon the metallic uranium, that it is impossible to obtain salts with them. The oxyd of uranium at the *maximum* is prepared by decomposing the nitrate of uranium by potash: it is of a lemon-yellow colour, and contains from 20 to 24 *per cent.* of oxygene; this oxyd cannot be brought to the state of acid by treating it repeatedly with nitric acid.

Potash, and carbonate of potash, do not dissolve the oxyd of uranium dried; but when these substances precipitate a salt of uranium, an excess of them re-dissolves it in part. This properly belongs more particularly to the alkaline carbonate. The same happens with ammoniac; but it appears that this alkali acts as a disoxygenator, for the yellow oxyd becomes more brown, and seems to pass into the oxyd at the minimum.

Olive

Olive oil disoxydates the yellow oxyd of uranium; it passes into a brown, and the oil becomes somewhat thickened.

Sulphate of Uranium.

The oxyde of uranium, obtained by the calcination of the nitrate heated to redness, boiled afterwards with dilute sulphuric acid, evaporated and dissolved afresh, presents a solution of a greenish-yellow colour, which crystallizes by spontaneous evaporation. A salt is obtained, of a lemon-yellow colour, the crystals of which seem to have the form of triangular pyramids, but when examined with a lens, they are found to affect the rare and singular form of pyramids with five angles.

The sulphate of uranium, heated to whiteness, is not fused, but entirely decomposed, and loses 36 *per cent.* There remains a black oxyd, which, dissolved in nitric acid, retains no sulphuric acid.

When exposed to the air and the rays of the sun, the crystals lose their transparency, without however efflorescing. It dissolves in five-eighths of its weight of water at 15°, whence results a liquid of a thick consistence, like syrup. It dissolves in twenty-five parts of alcohol; the solution undergoes some remarkable changes in the sun. The yellow liquor becomes turbid, and deposits a greenish-grey precipitate. After some days it acquires a strong smell of ether, much more resembling that of nitric than that of sulphuric ether. After having been filtrated the liquor is as limpid as water, and holds no more metal in solution. The precipitate is of a green colour, and retains some sulphuric acid.

In order to determine the relative proportions of the sulphate of uranium, the author dissolved 100 grains in water; after having accurately precipitated with muriate

of barytes, the sulphate obtained weighed when dried fifty-six grains, which correspond to eighteen grains of sulphuric acid. In order to determine the uranium, 100 grains of the sulphate of this metal dissolved in water were precipitated by potash; the precipitate, well washed and dried, weighed seventy grains.

A slight calcination, without decomposition of the salt, caused it to lose 12 *per cent.* of its weight, whence the author concluded that the sulphate of uranium was composed of sulphuric acid 18, oxyd of uranium 70, and water 12.

Nitrate of Uranium.

The solution of oxyd of uranium in nitric acid, evaporated spontaneously in a stove, gave quadrangular rectangular pyramids, an inch and a half in length.

Its colour is variable; it is a brownish lemon yellow, and the margins are greenish, sometimes the colour is deeper. When exposed for three days to the ordinary heat of the stove, and even by slightly calcining the crystals, it is converted into a yellow powder. 100 grains lost 14 of their weight; 100 grains of this nitrate being dried, dissolved in water, and treated by potash, the precipitate washed and calcined weighed 61 grains, whence it may be concluded that 100 grains of nitrate contains oxyd of uranium 61, nitric acid 25, water 14.

When the nitrate of uranium is heated, it first melts in its water of crystallization, then it becomes fixed, red, evolves oxygene gas and nitrous gas. There is left a yellowish-brown matter, disoxydated to that degree that it dissolves in nitric acid, with a disengagement of nitrous gas.

The nitrate of uranium, exposed to a dry air, at the temperature of 20°, becomes efflorescent; in a damp cellar

lar it deliquesces. It dissolves in half its weight of water. One part of alcohol can dissolve three parts and one-third of it; by a slight elevation of the temperature a part of the nitrate is decomposed in this alcoholic solution, some oxyd is precipitated, and ether is formed. The yellow solution, exposed to the solar rays, becomes green; a smell of nitric ether is exhaled, and a black matter precipitated.

Herewith the author concludes this memoir, of which he intends to publish the sequel hereafter.

Extract of a Memoir by M. EINHOF *on Vegetation, printed in the "Neues Journal dez Chimie," and translated by M.* VOGEL, *in the "Annales de Chimie."*

THE author has examined the action of several acids upon the vegetable, and has more particularly attended to that of the oxygenated muriatic acid and some others. He first observes, as a result of his experiments, that the oxygenated muriatic acid is very capable of making seeds germinate; but that it has no influence upon the young plants out of the earth.

He sowed cress-seed *(lepidium sativum)* in turf and sand, moistened with oxygenated muriatic acid concentrated to the maximum, and in other circumstances diluted with two parts of water. He likewise sowed the same kind of seed in sand moistened with water. Both were sprinkled every day with water and with concentrated oxymuriatic acid.

The seed moistened with the acid germinated eight, fifteen, and even twenty-four hours sooner than that which was moistened with water; the germs shot out

with uncommon rapidity, and in the space of twelve hours they attained to a length of six lines; but at this period their growth terminated.

The seed moistened with water attained scarcely to the length of half a line.

He poured upon cress-seed four parts of concentrated oxy-muriatic acid; at the end of six hours the germs appeared, and a disengagement of carbonic acid gas took place. The seed kept in water, on the contrary, did not germinate till at the end of thirty hours.

The metallic oxyds, especially the red oxyd of lead and the black oxyd of manganese, had been pointed out by M. Humboldt, as producing a powerful effect upon vegetation. The sulphuric acid had likewise been proposed by M. Blumenbach, as yielding its oxygene to the young plants. The author had no success with these substances, and he observes, that if any advantageous results are obtained with the acid, it is only because it combines with the calcareous carbonate of the soil, and forms a sulphate of lime, which is known to be a manure.

More might have been expected from the nitric acid, which gives up oxygene more readily. With this view the author sprinkled about two-thirds of a field sowed with lucerne *(medicago sativa)* with nitric acid diluted with eight parts of water. No sensible difference was observed during the first year; but in the second, the two-thirds of the field sprinkled with the acid distinguished themselves in a very striking manner; the height of the lucerne was much more considerable, it was greener and the stalks stronger.

The salts, though often recommended for promoting vegetation, do not present all the same advantages.

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The sulphate of iron is hurtful *. This assertion the author supports by several examples. He mentions, among others, that the employment of a certain kind of mud as manure, has always been found detrimental to vegetation. This substance yielded in analysis two *per cent.* of sulphate of iron.

A river which taking its source in the Hartz mountains, sometimes overflows its banks, carries with it, besides sand, martial pyrites, which it leaves upon the meadows which it inundates. The pyrites is converted into sulphate of iron, and after the floods, the soil becomes sterile, and yields afterwards hay of very inferior quality.

Gypsum, or sulphate of lime, which has long been considered as a manure, and employed chiefly for plants of the Diadelphia class, acts, according to the author, as a stimulus which promotes the respiration and exhalation of the plants †. The calcination of the sulphate of lime has no other purpose than that of favouring its pulverization. It is however adviseable to let it recover moisture previous to using it, for when placed in contact with wet plants, it forms a mortar which necessarily must stop up their pores.

The nitrate of potash is generally admitted to favour vegetation. Earth containing cress-seed was covered

* For an ample investigation of the effects of sulphate of iron upon vegetation, we refer our readers to Dr. Pearson's paper on this subject, inserted in our preceding volume, page 331.

† At the time when the author was engaged in this inquiry, he was no doubt unacquainted with the ingenious explanation which M. Parmentier has given of the effects of plaister of Paris as a manure. See *Nouveau Dictionnaire d'Histoire Naturelle*, articles *Cendre* and *engrais*. (Note of the French translator.) A paper containing M. Parmentier's observations and opinions on this subject is inserted in our preceding volume, page 60.

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with a layer of salt-petre; there soon took place a more rapid growth than in another quantity of earth to which this substance had not been applied.

This experiment, as well as the analysis made by the author of a very fertile earth, in which he found, besides argill and lime, nitrate of lime, proves that the nitrates in general are good manures.

Respecting the utility of the muriate of soda opinions are much divided. It is known that the Romans sprinkled sea-salt upon places where any great crime had been committed, with a view to render the ground sterile. In Egypt where the lands are covered with this salt, no vegetation takes place. The author put a very thin stratum of sea-salt upon a piece of ground; and he found that in small quantities it might be useful to vegetation.

Sulphate of soda, arsenic and the electricity of Volta's pile produced no satisfactory result upon vegetation, though it is probable that the electric fluid of the atmosphere has some influence upon vegetables.

The elastic fluids exhibited phenomenas so different from each other, that it was impossible for him to deduce certain inferences from them. The germination of cress-seed took place at once in oxygene gas and in azote, but in the latter the root-fibre did not exist in it.

The fibre of the root and the germ, after having burst the outer skin of the seed, shot out rapidly and vigorously in oxygene gas. Every two days the gas was converted into carbonic acid gas, and it became necessary to renew it often. The etiolation of the young plants under the bell-glass filled with oxygene gas, was of no long duration. The two first leaves, after having been well developed and green, began to languish, and died at the end of ten or twelve days. Azotic gas caused the seed to germinate, but there the growth ceased.

In

In carbonic acid gas, some seeds began to germinate, but the vegetation stopped.

Hydrogene gas exhibits a peculiar phenomenon upon cress-seeds; they become pale and covered with a tenacious mucilage of a fetid odour. None of the seeds germinated; they had entirely lost this faculty.

All these experiments were made both in contact with the light and in the dark; though the latter state is best adapted for germination, the young plants were pale and without vigour, whereas those which had received the influence of the light were very green.

The author is pursuing his researches with zeal, and promises to publish such of their more or less satisfactory results, as may prove interesting to the agriculturist and chemist.

Experiments on the Eudiometrical Means, and on the Proportion of the Constituent Principles of the Atmosphere.

By Messrs. HUMBOLDT *and* GAY-LUSSAC.

(Continued from Page 239.)

THE oxygene gas we obtained from the super-oxygenated muriate of mercury. To obtain it we employed a glass retort, to which had been soldered, by the flame of a lamp, the curved tube through which the gas was to pass; and, in order that we might have it as exempt from azote as possible, we filled the retort about one-fourth with water. This water being entirely reduced to vapour before the decomposition of the salt, soon expelled all the air out of the retort; but, in order to prevent the absorption which would have taken place before the disengagement of the oxygene gas, we plunged the extremity

tremity of the tube in a saucer filled with mercury, which we removed as soon as the gas began to be disengaged. In order that the oxygene may not, passing through the water, expel azote from it, we convey it directly into the upper part of the recipient which is to receive it, by means of a tube bent at a right angle, which at the one end ascends to the upper part of the recipient, and at the other is adapted to the first tube by means of a cork stopper, common to both. This process, which is very simple in its application, is particularly advantageous for the gases which are soluble in water, such as the carbonic acid gas, the azote oxyd gas, &c. We obtained our hydrogene gas by decomposing water by means of zinc and muriatic or sulphuric acid diluted with about six parts of water: we took care to fill exactly with acid the vessel from which the gas was to be disengaged, and not to make it pass through the water; but notwithstanding all these precautions, our oxygene left with the sulphuret four thousandth parts of azote, and the hydrogene, analysed by other means, shewed six thousandths. After these elucidations let us proceed to the questions which we have proposed to ourselves to resolve, beginning with this:

When we inflame a mixture of oxygene and hydrogene gas in the eudiometer of Volta, can the absorption of one of the gases be complete?

In order to ascertain whether all the oxygene or all the hydrogene could be entirely destroyed, we thought that if two gases were perfectly pure, or if we knew their degree of purity, and that their absorption must be complete, we should be able to find the same proportion for the principles of water, whether the hydrogene or the oxygene were predominant. In fact, by detonating mixtures of 300 parts hydrogene and 100 oxygene, and

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of 200 of the first and 200 of the second, in which the hydrogene and oxygene alternately predominate, and making the corrections due to the impurity of the gases, we obtained very nearly the same proportion. Although the absorption of the two gases might be complete, it were however possible that the proportions obtained, in making them alternately predominate, might not be identical, and this would be the case if, according to the predominance of either of the gases, there were formed an oxygenated or hydrogenated water; but since the proportions have become identical, it must necessarily be concluded, that the hydrogene and the oxygene were entirely absorbed. But though the absorption of the two gases may be complete under certain circumstances, it must not be supposed that it is so with any quantities; there are not only such proportions of hydrogene and oxygene, or of their mixture with azote, or even with any other gas, that it is impossible to inflame them by means of the electric spark; but there are also others with which the inflammation having been commenced, stops before the combustion is completed. We proceed to cite experiments to this effect, which appear to us to be conclusive.

We mixed 100 parts of hydrogene with 200, 300—900 of oxygene, and inflamed them by the electric spark: with these different proportions the absorption constantly amounted to 146 parts; but with 1000 of oxygene it was at once reduced to 55; with 1,200 and 1,400 it was reduced to 24 and 14, and with 1,600 it was reduced to 0; that is to say, no inflammation took place. These different results are exhibited in the following table.

Hydrogene.	Oxygene.	Absorption.
100	200	146
100	300	146
100	600	146
100	900	146
100	950	68
100	1000	55
100	1200	24
100	1400	14
100	1600	0 *

What is striking in these different experiments is to see, 1, a constant absorption, with very different proportions, change suddenly into a decreasing absorption; 2, the combustion of hydrogene gas which had commenced, stop before it was completed; 3, that there are such proportions of hydrogene and oxygene as cannot be inflamed. These different phenomena will be in some measure explained in the sequel; but in the mean time we shall remark, that there are even very extended proportions with which the combustion of the hydrogene gas may be complete.

The above-mentioned phenomena are not peculiar to the hydrogene and oxygene gases mixed together, under the circumstances of which we have been speaking: they also take place when we inflame 100 parts of oxygene with 200, 300—1000, &c. of hydrogene; only it then happens, that the term when the absorption ceases to be constant is more remote; and to comprehend the reason

* The absorptions 68, 55, 24, and 14, are possibly not exact within two or three hundredths, for our instruments being too small for the corresponding proportions, we were obliged to measure them several times; but this is of no moment with respect to the phenomenon in general.

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of this it is sufficient to observe, that in this case there disappear about 300 parts by the inflammation, whereas there disappeared only half the quantity in the preceding experiments.

Azote gas and carbonic acid gas present also analogous results. If, for example, we inflame a mixture of 900 parts of azote, 100 of hydrogene, and 100 of oxygene, the absorption, which ought to be 146 parts, if the combustion were complete, was in one experiment, which is that which we take as example, only 50 parts, though in others we have seen it a little above or below this quantity. With inferior proportions of azote, we have constantly had the same absorption of 146. Although the azote appears here to comport itself like the oxygene, since with 100 of hydrogene and 1000 of oxygene, we had nearly the same result as with 100 of hydrogene, 100 of oxygene, and 900 of azote, we shall not draw from hence any inference, because we have not sufficiently multiplied and varied our experiments. Nevertheless, those which we have made tend to prove, that when determinate proportions of oxygene gas and hydrogene gas are mixed with different gases, the absorption may be constant as far as to a certain point, beyond which it diminishes very rapidly.

The absorption of the oxygene and of the hydrogene being complete in determinate proportions, and not so in others, it will always be possible, when a gaseous mixture is given, which alone would not be able to inflame, to reduce it to another with which the absorption of one of the gases would be complete, by adding to it oxygene or hydrogene, or even both together.

The combustion of the 100 parts of hydrogene in the preceding experiment not having been complete, we analyzed the residue. 100 parts, placed in contact with

phosphorns, diminished by 7 in the space of four hours, an evident proof that the residue contained oxygene. In order to ascertain whether it had retained hydrogene, we inflamed, in Volta's eudiometer, a mixture of 200 parts of the preceding residue, 200 of oxygene gas, and 200 of hydrogene gas; in all 600 parts. After the inflammation 312 parts had disappeared; and as, according to experiments of which we shall give an account hereafter, 100 of pure oxygene require for their saturation 200 of hydrogene gas, the absorption which, with the hydrogene gas which we have employed, ought only to have been 292 parts, amounted to 312, the residuum must necessarily have furnished a sufficient quantity of it to carry the absorption from 292 to 312; that is to say, it must have contained 13,3 parts. Now calculation shews that it ought to have contained 12; it is therefore clearly proved, that though inflammation took place, the combustion was not complete; and that all the hydrogene did not enter into combination, since we have found that which had not been absorbed in the residuum. We must observe, that in all cases in which the absorption was not complete the inflammation was languid.

From comparing, in the inflammation of hydrogene and oxygene gas, the effects of electricity with those of a high temperature, we have been led to believe that the inflammation produced by the electric shock might very likely be owing to the heat produced by the instantaneous compression which the electric spark occasions in its passage. In fact we know, from our own experience, that the inflammation of a mixture of hydrogene and oxygene gas depends solely upon the temperature when this inflammation is produced by heat. For if we cause this mixture to pass very slowly through a tube heated very gradually, from its extremity to its central part, without opposing

opposing the free dilation of the gases, the inflammation will take place as soon as the temperature shall be raised to a sufficient degree. This being admitted as fact, that the inflammation of the oxygene and the hydrogene gas takes place only at a certain temperature, let us see what passes in their inflammation by the electric spark. When this passes through a mixture of oxygene and hydrogene, it displaces it by its rapid passage, which does not permit the gaseous particles to communicate to each other the motion as quickly as they have received it; hence results a very strong instantaneous compression, which produces an elevation of temperature superior to that which is requisite for the combination of the gases, and the inflammation being thus commenced, must be propagated very rapidly.

According to this mode of accounting for the effects of electricity, we thought that when a weak spark produces only an imperfect combustion in a mixture of hydrogene and oxygene gas, a stronger one would produce a more complete combustion; but whether it was that we did not employ a sufficient brisk electricity, or that we did not multiply our experiments sufficiently, we obtained no sensible differences in employing the spark of an electrophorus, three decimeters in diameter, or the shock of a highly-charged Leyden flask; but the construction of our eudiometer did not permit us to draw very brisk sparks, on which account we shall reserve our opinion respecting the influence of the force of the electricity in the inflammation of the hydrogene and oxygene, till we shall have made farther researches upon the subject.

In the above-described experiment on the inflammation of a mixture of 900 parts of azote, 100 of oxygene, and 100 of hydrogene, the absorption was not so considerable as it ought to have been, and we have proved that

that the residuum ought to contain what had escaped combustion; that is to say, it ought to be composed of six parts of hydrogene, eight of oxygene, and eighty-six of azote in the hundred. Therefore, since the combustion was interrupted when these proportions took place, it may be concluded that another electric spark would no more be able to inflame this mixture. Consequently, in the atmosphere, which contains much less than six hundredths of hydrogene, the electric spark will not be able to inflame it, or if it does it at the place of its passage, by reason of its great force, the inflammation will not be able to propagate itself, but will be in a manner confined to the places which it traverses. Hence, finally, the inflammation of hydrogene gas, by lightning, and *à fortiori* by weaker charges of electricity, will not serve to explain the igneous meteoric phenomena; or if these phenomena are actually results of the inflammation of hydrogene gas, we must conclude that there are more than six hundredths of it in the air at the moment when they are produced; which is contrary to all probability, especially when we recollect that air, collected at a very great elevation, presented no appreciable quantity of hydrogene above that contained in atmospheric air collected at the surface of the earth.

But if every time that we cause an electric spark to pass into a mixture of hydrogene and oxygene, or of azote, hydrogene, and oxygene, which is not capable of inflaming, there is actually produced a local and instantaneous heat by the compression which the spark occasions in its passage, it is possible that by directing a succession of sparks into one of the mixtures of which we have been speaking, a slight local inflammation might be produced each time upon the passage of the spark, and that thus it might be practicable to destroy a determinable

able quantity of hydrogene inveloped in a large proportion of azote and oxygene, or of oxygene only. What seems to confirm this supposition is, that it is well known that ether and ammoniac, which are decomposed by heat when they are made to pass in vapours through a red-hot tube, are likewise decomposed by repeated electric shocks. It would also be interesting to know, whether it be possible to inflame by the electric spark a proper mixture of oxygene and hydrogene, after having dilated it by means of the pneumatic machine. If its inflammation by the electric spark really depends upon the heat which this produces by compression, it would be natural to suppose, that when these gases are dilated, the compression by the spark being less considerable, the heat which is produced by it must also be much slighter, and that there may be a degree of dilatation of the gases at which the inflammation cannot take place. We have not yet had time to try these different experiments; but we do not abandon our design to attempt them, which we hope we shall be able to do shortly.

To recapitulate: there exist certain proportions of hydrogene and of oxygene, or of these two gases with azote, in which combustion can be complete. There exist also others, in which it stops spontaneously before being completed; and, finally there are proportions in which it cannot take place at all. The hydrogene gas which escapes the combustion is found again entire in the residue. When we cannot produce by the electric spark a complete inflammation of the hydrogene gas, or even when we cannot commence it, nothing more is necessary than to augment the proportions of the oxygene or of the hydrogene. The igneous meteoric phenomena cannot be the result of the inflammation of the hydrogene gas, because in the regions where the principal of them

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are supposed to take place, such as the sudden and abundant torrents of rain which sometimes succeed a clap of thunder, it would be necessary that there should then be more than six hundredths of hydrogene in the atmosphere, without which the inflammation could not take place; besides which, only the quantity exceeding this proportion could pass into inflammation.

We may account for the cases in which the combustion was not complete according to the laws of the affinities, by saying, that when one of the gases becomes very predominant it may defend the other by its affinity, and guard it in part from combustion. Although this affinity may be very weak, we conceive, with M. Berthollet, how the quantity of gas may compensate for it; and if there be in the different gases peculiar properties of stopping the combustion sooner or later, this may be explained by their different nature. But when we consider the case in which the hydrogene is mixed with oxygene only, and suppose the phenomena of its combustion with different proportions of oxygene to depend upon affinity, how explain the sudden transition from a constant absorption to a decreasing absorption, when it is agreed that if the hydrogene can be prevented from the combination by the oxygene, the effect of the latter must follow a regular law? How conceive that these two gases, after having been placed in circumstances favourable to their combination, can by their affinity maintain themselves in their elastic state, when they might form a much more dense combination, namely, water? How conceive, finally, that an affinity which produces a very great condensation and saturation, can be inferior to an affinity which produces no change in the dimensions of the two gases, no saturation? Hydrogene and oxygene, in whatever state they may be, have the same degree of affinity, as

this

this affinity is measured by their capacity of saturation; only the state in which they are may be more or less favourable to their combination. Now, to say that hydrogene and oxygene have a greater affinity in the state of gas than in the liquid state, is to say that their molecules attract each other more when they are very remote than when they are very near to one another. These objections against an explanation founded solely upon the affinities, having appeared to us to be of some weight, we have endeavoured to present one which, in our opinion, did not involve the same difficulties.

All combustible bodies require in general a certain elevation of temperature, in order to combine with oxygene. Carbon, for example, is not converted into carbonic acid until it is red-hot; and this same substance, which at a high temperature can continue to burn when it is exposed to a current of aqueous vapour, is extinguished as soon as it is immersed in water. This principle, that bodies in general require a certain elevation of temperature in order to burn, being once admitted, let us suppose that we have a body which burns in a given volume of atmospheric air, and that the temperature necessary for the combustion is maintained solely by the heat due to the absorption of the oxygene: let us also suppose, that at the commencement of the combustion the heat due to the fixation of the oxygene contained in a cubic centimeter of air is $=1$, and that the heat lost during the fixation, whether in radiating heat, or by the absorption which is made of it by the azotic gas or other bodies, is $=\frac{1}{4}$, not taking here into consideration the law according to which it decreases. According to these premises, it is very evident that in the first moments of the combustion the temperature of the body must rise; but, in pro-

portion as the quantity of oxygene shall diminish, and that of the azote proportionally increase, the heat communicated will also diminish. A period will therefore arrive, at which the heat lost will be equal to the heat communicated, and below which, the temperature being too low, the combustion must cease. What evidently proves that the combustion stops only because the temperature is too low, is that if we artificially maintain the temperature sufficiently high, the body will continue to burn.

Now this explanation will still hold good, when, instead of azote, sulphurous gas, hydrogene gas, carbonic acid, or any other gas, is mixed with the oxygene; only the combustion may cease sooner or later than with azote gas. For it is very evident, that if the sulphurous gas, or carbonic acid gas, had a capacity for caloric, much greater than that of azote, supposing them to be mixed with oxygene in the same proportions as the latter, the loss of heat would be much greater, and consequently the cessation of the combustion must take place sooner. But if the gases had equal capacities for caloric, they must all of them stop the combustion at the same period, as we have seen has nearly been done by oxygene and azote with hydrogene, and this would perhaps afford a solution of the important question, whether the gases have equal or different capacities.

Thus a combustible body, sulphur for example, would cease to burn in a determinate volume of air, not because the affinity which the azote or the gases produced have for oxygene were more powerful than that of the combustible body; but because the heat absorbed by these gases, which tend to place themselves in an equilibrium of temperature with the burning body, would be greater than the heat proceeding from the fixation of the oxygene;

ygene; whence it would result, that the temperature would soon be reduced below that necessary for the combustion. In fact, we know that sulphur can continue to burn in air in which it had been extinguished if we raise the temperature to a sufficient degree.

What takes place in the instantaneous combustion of the hydrogene in Volta's eudiometer, is perfectly analogous to what passes in its successive combustion in a given volume of air, or in that of any other body. If we place a lamp, the flame of which is supplied by hydrogene gas, under a bell-glass filled with oxygene gas, the flame will become small, bright and slightly coloured. If we replace the oxygene by atmospheric air, the flame will be more voluminous, less bright, and more coloured. In proportion, especially as the relative quantity of oxygene diminishes, the flame will increase in size, because the hydrogene will be obliged to go farther to come at the oxygene, and the flame will soon be extinguished, although the air still contain some hundredth parts of oxygene. The phenomena which take place in Volta's eudiometer are of the same nature. When the proportions of oxygene and hydrogene do not deviate much from those which constitute water, the flame is still very bright, notwithstanding its dilatation; but if we mix, for example, 1000 oxygene with 100 of hydrogene, the flame is then weak, of a blueish-green colour, and the combustion of the hydrogene is far from being complete, for we still find nearly two-thirds of it in the residue. What farther proves that the combustions' not having been complete was owing to the temperature not having been sufficiently elevated, is that if the residue is made to pass, as was done by us, through a red-hot tube of porcellain, the whole of the hydrogene will be absorbed.

We must observe, that in the combination of the hydrogene and oxygene gases, a very singular phenomenon takes place, which has long since engaged the attention of M. Monge.

"How does it come to pass," says this distinguished philosopher, "that by elevating the temperature of the two gases, or, in other words, by increasing the proportion of the solvent, we diminish the adhesion which it had with its bases?" Far from conceiving that the present state of our knowledge is competent to give a satisfactory solution of this question, we wish to recommend it anew to the attention of philosophers. In fact, according to the idea which we are able to form to ourselves of the force which produces the combinations, and of those which are opposed to it, the elastic state indicates that the force of cohesion is destroyed, and that two bodies in this state are in the condition most favourable to combination; so that now that the attractive force of their molecules has been changed into a repulsive force, every cause which shall favour the latter will be in opposition to the former. It happens, however, that by raising the temperature of the two gases, that is to say, by augmenting their repulsive force, their attractive force is increased. It cannot be believed, that the heat does nothing more than separate their molecules to greater distances from each other; for, in this case, why should a mixture of hydrogene and oxygene gas not inflame under the receiver of an air-pump, where it may be indefinitely dilated? Neither is it to be supposed that the heat acting instantaneously can produce a compression which favours the combination of the two gases by bringing their molecules into closer contact; for it is easy to convince ourselves that a mixture of oxygene gas and hydrogene gas, heated very gradually without

without opposing its dilatation, will nevertheless inflame when the temperature shall be sufficiently elevated.

Having now proved, that under determined circumstances the combustion of hydrogene and oxygene may be complete, we shall proceed to examine whether its products are constant.

According to all the experiments which have been made on the composition of water, the result has been considered to be uniform. There has, however, sometimes been obtained a small quantity of nitric acid; but it has been ascertained that this acid is not a constant product of the combustion of the hydrogene, but, on the contrary, merely accidental. Cavendish was the first who discovered this formation of nitric acid, and Messrs. Fourcroy, Seguin, and Vauquelin, have taught us how we may avoid it, and obtain water without any acidity. It has not, indeed, been demonstrated, that oxygenated or hydrogenated waters have not been formed, because, in all the exact experiments that have been made, the combustion of the hydrogene gas has always been effected in the same manner, and yet most it would be proved that those which have been obtained are constant under the same circumstances. If we compare the combustion of hydrogene gas with that of nitrous gas, the products of which are so variable, we shall be still more justified in concluding, that since oxygene has always predominated in the experiments which have been made, an oxygenated water may have been formed; whereas, if hydrogene had been predominant, a hydrogenated water would have been the result. Let it then be admitted, that an oxygenated water may be formed; if, for example, we obtain it in all circumstances, and it be constant, this will be of no consequence with regard to the

proportion

proportion of its principles, which must serve for the analysis of the air; but if it be so only because the oxygene predominated, it is manifest that we shall no longer obtain the same proportions when we alternately cause both gases to predominate.

TO BE CONTINUED IN OUR NEXT.

Intelligence relating to Arts, Manufactures, &c.

(Authentic Communications for this Department of our Work will be thankfully received.)

A Varnish for Wood and other Substances.

M. CHEVALIER, of Berlin, has invented a new varnish applicable to wood, plaster, and other substances. The specimens which he has submitted to the examination of M. Hermbstaedt, have resisted the action of river water, of water impregnated with salt, of the air, of rain, and of a weak alkaline ley. M. Hermbstaedt is convinced, that wood employed for the construction of ships or houses, piles sunk in water or wet earth, &c. if coated with this varnish would last three times as long, and run no risk of rotting. Two pounds and a half of the varnish are sufficient for a surface of a hundred square feet. It is sold at eighty-two Prussian dollars the hundred weight.

Formic Acid.

M. Suersen, of Kiel, has published some experiments upon the formic acid; from which he concludes that, notwithstanding the labours of M. Deyeux, and the later ones of Messrs. Fourcroy and Vauquelin, the identity of this acid with that of vinegar is not proved.

Painting

Painting on Glass.

M. Mohe, an optician at Berlin, is said to have restored the art, which had so long been lost, of indelible painting upon glass. According to the German Journals, the celebrated chemist Klaproth has declared that M. Mohe's painting cannot be effaced without destroying the glass.

Galvanic Pile composed entirely of Vegetable Matters.

Dr. Joseph Baronio, of Milan, has published a description of a Galvanic Pile, composed entirely of vegetable matters. He cut disks of horse-raddish and of red beet, about two inches in diameter; he then prepared equal disks of walnut wood. The latter disks were furnished with raised margins, to contain a small quantity of solution of acidulous tartrite of potash (cream of tartar) in vinegar, in which liquid they had previously been boiled, in order to free them from the resinous principle which the walnut-wood contains.

The pile being formed with sixty pairs of disks, the one of horse-raddish, the other of beet, separated by the wooden disks, into which a small quantity of the above-mentioned solution is poured, Galvanic effects are obtained upon a prepared frog, the spinal marrow of which is made to communicate, by means of a leaf of *cochlearia*, with the base of the pile.

In place of the raddish and red beet, the author has already substituted disks of other vegetables with equal success. He invites men of science to repeat and vary these experiments, which, he flatters himself, will serve to extend the application of the Galvanic theory to all vegetation.

List

List of Patents for Inventions, &c.

(Continued from Page 240.)

JOSEPH FLETCHER, of Horsley, in the county of Derby, Needle-maker; for a machine for raising water. Dated January 23, 1806.

GEORGE BARTON ALCOCK, of the city of Kilkenny, in that part of the united kingdom of Great Britain and Ireland called Ireland; for certain improvements in lamps. Dated January 23, 1806.

JOHN DOBBS DAVIES, of New Compton-street, in the county of Middlesex, Gentleman; for a saddle-bar on an improved construction, which he denominates the Motion Saddle-bar. Dated January 23, 1806.

ROBERT BERRIMAN, of Speen, in the county of Berks, Wheelwright; for a machine for preparing land for the reception of seed, which he is confident will prove of the utmost advantage to agriculturists in saving corn, in producing a regular and more abundant crop, and in enabling the farmer, at an easier rate, to keep his land free from all kinds of weeds. Dated January 23, 1806.

ERRATA.

Page 256, line 13, *for* chain or flexible substance turning, *read* sliding piece, which I call a pusher.

258, 30, *for* of looks *read* by cocks.

251, 6, *for* passes *read* passes through, and lines 8 and 19 *for d read* D.

THE

REPERTORY

OF

ARTS, MANUFACTURES,

AND

AGRICULTURE.

No. XLVII. SECOND SERIES. April 1806.

Specification of the Patent granted to JAMES BOAZ, *of the City of Glasgow, Civil Engineer; for a new and improved Method of raising Water, and working Machinery by Means of Steam.*

Dated July 2, 1805.

With a Plate.

TO all to whom these presents shall come, &c. NOW KNOW YE, that in compliance with the said proviso, I the said James Boaz do hereby declare, that my said invention is described in manner following; that is to say: My method consists in constructing and using of a certain engine or instrument, which in this my specification I do designate or denote by the name or denomination of a *Pump*; which said pump consists of parts capable of being varied as to their relative dimensions, materials, forms or shapes, situations, and, in some respects, their uses or applications, still preserving the distinctive character of the engine or instrument itself. And I do hereby fully describe the same, by reference to

the drawings or figures hereunto annexed, with several of the most obvious and direct methods of varying its structure, sufficient to enable any workman, of competent skill for works of this nature, to carry my said invention into effect and practice.

A, Fig. 1 (Plate XIII.) is the tube through which the steam passes from the boiler to this pump or engine; B is the upper compartment of the pump; C the lower compartment. These are divided horizontally by *dd*, a partition into which is fixed *e*, a tube open at both ends: *ff* is a floating piston, which rises and falls with the surface of the water in C, and is perforated throughout in the middle so as that it may rise and fall in C without being stopped by the tube *e*. Fig. 2 is a perspective section of the piston. To the bottom of the piston is fixed *g g*, a rod, which passing through the centres of *e* and B, works through *h*, a stuffing box. The upper end of *g* is employed to work the hand-gear which is not here drawn, as it may be variously constructed like that of other steam engines. I is the hot water cistern; J is the cold water cistern; K the hot water tube; L the cold water tube; at the upper end of which, and within J, is *m*, the delivering valve. L and K are joined together at *n*, uniting into O O, one tube, the lower end of which is immersed in the water to be raised, and is furnished with *p*, the receiving valve in the well; *q* is the exit spout for the discharge of the water raised; *r* is the valve through which steam passes from the boiler into C; S the eduction valve communicating with B and C; *t* the breathing valve; *u* the returning cock; V the float of *u*; W the height of the ascent of cold water; X an additional pipe. In all the figures steam is represented orange, water blue *.

* The colouring is necessarily omitted in the plate, and the parts containing steam are not shaded.

Operation.

Operation. The first thing to be done before working, is to fill B, C, L, K, O, O, I, J, with water; the steam is then admitted through *r* into C. This in the next place heats the water. When it has approached the boiling point, its surface in C being pressed by the steam, and the water not being allowed to escape elsewhere, is forced up the tube *e* into B, which being still full of water, a part passes down K, and the valve *p* being shut, the water ascends in L, and opening the valve *m*, spreads itself in J, whence it finally escapes through the spout *q*. When the steam has displaced the water in C, as represented in the figure, the piston (always descending with the surface of the water in C) then acts by its rod upon the hand-gear which shut the steam valve *r*, and opens the eduction valve S; the consequence of this is, the water in B, by its own gravity, runs down *e*, and forces the steam in the lower compartment C to pass through S, into the upper compartment B; wherein, ascending by its own levity, it comes into contact with the bottom of the cold water cistern J, and is condensed; a vacuum being thereby formed, the atmosphere, to restore the equilibrium, forces cold water from the well up through P, O, O, K, as far as the dotted lines at W. By this time the piston will have risen to *d d*, when it will cause the hand-gear to instantly shut S and open *r*; through this latter, fresh steam from the boiler will enter C for another stroke, as before. Should any air have been injected along with the steam of last stroke, or otherwise, it will have ascended to, and be lodging at, the top of B. In order to get rid of this air, which if allowed to remain would affect the vacuum next stroke, the very small breathing valve *t*, is placed on the top of B, opening upwards into I, where it is always covered with water: whenever therefore fresh steam presses into C, the pressure opens *t*, allowing the air to escape. A small

quantity of hot water always passes out of B, along with it. This hot water would accumulate in I, and run over, but the cock *u* is opened by its float V whenever the hot water in I exceeds a certain height, and the superabundance is taken again into B. By this mode of operating no cold water from the well is ever allowed to enter B or C, for all that ascends into K at each stroke by its greater gravity than hot water keeps undermost, and rising no higher up that pipe than to W, descends again and ultimately passes away by the spout *q*. The vacuum produced is also very perfect, for the pump is most completely purged of air through the breathing valve every stroke. The strength of steam necessary to force the water down K and up L every stroke is not great, for the columns of fluid in these pipes nearly balance each other, and there is little more power required than what is necessary to overcome the friction of the water on the sides of the pipes.

When it is required to force water above the pump, after it has been so raised by the atmosphere, the spout *q* is closed up, and the additional tube X for the water to pass up through is fixed upon the top of the cold water cistern. The pump then works as before, only the steam must be strong in proportion to the height the water is required to be raised. In both cases the boiler should be fed out of the hot water cistern.

When water is to be raised a small height only, say 15 feet, the pump may be wrought with very weak steam, in exactly the same way as Fig. 1, with this difference: the delivering valve is placed at *m*, Fig. 3, from whence the water raised runs off through the spout *q*; J is the cold water cistern, I is the hot water cistern, in which there is neither breathing valve, returning cock, nor float, as in Fig. 1. Instead thereof, however, there

is

is *y*, an air chest, furnished with two cocks *z* and X, the upper end of *z* opens into the cistern I, and is always covered with water; its lower end opens into *y*. The upper end of X opens into *y*, and its lower into B.

Operation. When steam enters C, cock *z* is shut, and cock X is open. The consequence is, whatever air may have been in B will by its levity ascend up through X into *y*, and displace its bulk of water. (The air in *y* is coloured red *.) When the vacuum takes place X is shut and *z* open: the consequence is, all the air that had so arisen into *y* will ascend through *z* and escape, as its place will be occupied by as much water. The opening and shutting of cocks *z* and X may be performed by the motions of the piston rod, and will allow an escape of air from the pump every stroke. It is perhaps unnecessary to add, that the cistern I and J will require a continual supply of water, which may either be raised into a reservoir by shutting *n*, and working for a time, as Fig. 1, or otherwise, as found most convenient. When a quantity of oil or fat matter is allowed to float on the water in B and C, Figs. 1 and 3, it more completely prevents the water therein from being cooled by being in contact with the bottom of the cold water cistern. Instead of forming the body B, C, of the pump, Figs. 1 and 3, as there drawn, one vessel placed above another, and communicating by cocks or valves, might answer the same purpose, though perhaps not quite so well, and the hand-gear made to work by a floating piston on the outside, buoyed up every stroke by the water emitted from the delivering valve *m*.

When water is to be raised from where the pump is placed, as, say from A B, Fig. 4, it may be con-

* In the plate this part is necessarily not coloured, and is left unshaded.

structed

structed as follows: B B, is the body of the pump; *r* is the steam valve; *f f*, the piston, which is double, the more effectually to prevent any cold water ascending far up in B; indeed it may consist of more pieces than two, or a quantity of oil or fat matter in B will serve as a barrier between the hot and cold water; *g g* the piston-rod working through the stuffing-box *h*, to direct the hand-gear; *p* is the receiving valve; *m* the delivering valve; X the pipe for the water to be raised through; *t* the steam-escape cock.

Operation. The whole being filled with water, shut *t* and open *r*, the steam enters B; and after heating the water therein, it presses upon its surface. This pressure shuts the valve *p* and opens *m*, forcing the water up the tube X. When the piston has descended sufficiently far, *r* is shut and *t* opened; the consequence is, the water from A B presses down the valve *p*, and as it cannot, for want of height or head, raise the valve *m*, it ascends into B, say as far as to W, presses up the piston *f f*, and discharges the waste steam through the cock *t*. The steam-cock *t* is then shut and *r* opened; the pressure of the fresh steam shuts *p* and opens *m*, and forces up the water through X as before. By this mode of operating no cold water ever ascends higher into B than to W, and it is again pressed down, to be alternately forced away. In place of allowing the steam to escape at *t* it may be condensed or used for other purposes.

Fig. 5. is another construction of this pump, by which water can be raised without condensing the steam. B is the steam cylinder; *r* the steam feed cock; S the waste steam cock; *f* the floating piston; *g g* the piston-rod, which works the hand-gear; K the hot water pipe; L the cold water pipe; *p* the receiving valve immersed in the well; *m* the delivering valve; *q* the exit spout; *t* the

breathing

breathing valve; *u* the cock, which regulates the size of the aperture, through which the breathing is made; J the cold water cistern; A the rarifying vessel or exhauster. Dark blue * represents mercury, or any metallic composition fluid in boiling water.

Operation. The whole being filled with water and mercury, *per* figure, shut S and open *r*; the steam from boiler rushes into B, and after heating the water therein, it presses on its surface, forcing the mercury out of B up into A (where it now is). The mercury in A forces the water above it down the pipe K up L, and delivers the water through *m* into the cistern J, whence it runs out by the spout *q*. When the piston has been sufficiently far pressed down (in which state it is seen in the figure) its rod shuts *r*, and opens S; consequently the mercury which had been forced up into A being now allowed to act by its gravity, in its descent again into B, performs three operations at the same time. First, it raises the floating piston; secondly, it presses out the old steam through the waste steam cock S; thirdly, by its gravity it produces a vacuum in A, which the atmosphere fills with water from the well through *p*. Fresh steam is then admitted as before. Any globules of air that may have reached the top of the bend of the hot water pipe collect themselves together, and are emitted through the breathing valve *t* every time the steam enters B. In case it is seen that too much water passes through *t* along with the air, the cock *u* must be turned to diminish the passage; an additional pipe, as X, Fig. 1, may be fixed here on the top of J for raising the water still higher. It is adviseable in all cases or constructions, when convenient in other respects, to place the body of the pump, or as much of it as possible, within the boiler, as this prevents

* The mercury is shewn in the plate by the dark shading.

waste

waste of steam, and saves casing, which the pump ought to be surrounded with.

When it is required to raise water several hundred feet, the pump is constructed as follows, and the water is raised by stages of twenty feet, less or more, by a series of vessels and pipes in the pit, similar to a method invented by Mr. Peter Keir, and long ago published by him; and to which series I claim no exclusive privilege, but only describe the same as a suitable addition to be used along with my said invention.

Description of the Steam-pump adapted to raise Water by two or more Stages. (See Fig. 6.) The dark space A is the pit; B B is the surface of the earth; C a cavity below the surface for the steam-pump; G is the rarifying vessel; H is the main air-pipe, into which no water ever enters. The other pipes are all for water. This drawing is taken at the moment when the steam is seen rising up from I into J to its own destruction. The water is represented as returning back from G into I to fill up the vacuum occasioned by the condensation of the steam. At the same instant the water appears rising up through the water pipes, as will be explained.

Statement of the process. When by the admission of steam from the boiler into I, the rarifying vessel G has been filled with water forced out of I, and if that steam be afterwards condensed, the water in G will return back into I, whence it came; hence the air then in H and G, as well as in the air-tight vessels K L M N, will thereby be very considerably, and in all alike equally rarified. O is a foot valve at the bottom of the pit; P Q R are three open tubs placed below L M N; S is the cold water cistern placed below K. Now it is obvious, that in consequence of the rarifaction having taken place in K L M N, the atmosphere will endeavour to restore an equilibrium, and will force water from O up into N, from R into M, from Q into

Q into L, and from P into K. Fresh steam from the boiler is then admitted into I; it displaces the water therein, forcing it up again into G. Hence the air contained in G, as well as in the air pipe, and in K L M N, (which is nearly of the same density as the atmosphere) is endeavoured to be compressed; the consequence is, the water from K L M N, by its own gravity, descends through the respective foot valves of these vessels into S P Q R; that which falls into S runs off by a declivity on the surface of the earth, but what falls into P Q R remains there ready to be forced up another stage when the next rarefaction takes place.

It might perhaps be found a saving, if each of the water pipes had a foot valve, to prevent the return of a portion of the water at each stroke; but these are not drawn in the sketch. The air is represented red *.

Calculation. Suppose the four close vessels K L M N, contain each ten gallons, the rarifying vessel forty gallons; the contents of the air pipe and its branches being trifling are, to avoid confusion, not taken into account. If the rarifying vessel be filled with water, and that water withdrawn from it, the air contained therein and in the four small vessels will be rarified exactly one half, so that if a barometer tube was interposed between any of these vessels and the atmosphere, the latter would raise the mercury therein just about fourteen inches; that is, with a pressure equal to seven and a half pounds per square inch. Hence water would naturally rise into these vessels from a depth of sixteen feet, and ten gallons would be delivered on the surface of the earth at every stroke. If the rarifying vessel was made larger, the degree of rarifaction would of course increase in a reciprocal ratio, and water would rise from a greater depth; but in every case

* See note, page 325.

water is raised bulk for bulk of the steam that enters into I. To make these drawings more clear, several parts are shewn in profile, which in practice will answer better if turned in other directions, whilst other parts are not in just proportion to the rest, it being there meant to shew the general effect and connexion of the whole. The close vessels K L M N, need be but small; the open vessels P Q R may be stout wooden tubs, and will cost but a trifle each; the valves are few, and if made of metal will last long. There is no friction elsewhere that need be taken notice of.

When I have occasion for blowing air into a furnace, or for other purposes, I cause the pump to produce a blast at the same time that it is raising water, thus *p*, Fig. 7, is the receiving valve of the pump, through which the water raised passes up by the pipe O; D is the air vessel, placed below the surface of the water in the well, having a pipe, 1, 1, leading therefrom to the furnace.

Operation. When the vacuum takes place in the pump, the double valve *p* rises, shuts the lower passage 2, while the atmosphere forces air through the air valve 3, down the pipe 1 1, and the pump is supplied through the pipe O with water out of D, and not out of the well; consequently the surface of the water in D is depressed in proportion to the quantity of water taken thereout. When fresh steam enters the pump, the double valve *p* falls; consequently the water in the well, seeking to find its level, passes through the aperture 2, now open, fills D ready for the next stroke, and forces the air out of it through the other air valve 4, to blow the furnace. The blast may be made constant by any of the usual modes. Instead of placing D at the bottom of the well I sometimes place it at or near the mouth of it, and by alternately filling and emptying D, I produce a similar blast, after which the water raised is applied to other purposes.

When

When I have occasion to raise but little water, I cause the whole power of the pump to be exerted in producing a very strong blast, thus: J, Fig. 8, is the cold water cistern of the pump Fig. 1, having the spout *q* closed up; 1, 2, 3, 4, are air vessels; 5, 5, 5, are air pipes leading from 1, 3; 6, 6, 6, are air pipes leading from 2, 4. The air confined in the pipe and vessel is coloured red *.

Operation. When a vacuum takes place in the pump, in which situation the figure is represented, the hand-gear of the connecting rods 7, 7, shuts the valves of J, 2, 4, and opens those of 1, 3; the consequence is, the water descending from 1, 3, into 2, 4, forces the air out of the latter up through the air pipes 6, 6, 6; at the same time that 1, 3 are replenished with air through the air pipe 5, 5, 5. When fresh steam enters the pump, the hand-gear opens the valves of J, 2, 4 and shuts those of 1, 3; the water then falls out of J, 2, 4, into 1, 3, and the well, and so on alternately. Hence each portion of water performs four operations from the time it leaves the cold water cistern till it reaches the well; and it may be made to do more or less, in proportion to the strength of the blast wanted. But by four stages, a blast of upwards of three pounds pressure upon the square inch may be in general easily obtained.

As from the different situations in which I use my pumps or engines, the water to feed my boiler sometimes requires to be raised a considerable distance through the feed pipe 1 1, Fig. 9, and as I have often occasion to work with steam of a high degree of expansibility, I feed my boilers in the following manner. A is the boiler; B a vessel in which and in the boiler, the water is common; it is connected with the boiler at the top by an open tube C, through which the steam has constant access to B, and at

* See note, page 325.

the bottom with another tube, having a register cock in *d*. E is a vessel connected with the boiler by a tube *f*, in which is a cock *g*, wrought by the tumbler *h h*, which consists of two levers, fixed in a nave, that runs on a shed on the centre of *g*. A pipe *i i* reaches from the bottom of E, and terminates in a valve J, which opens upwards; a branch K leads from *i i* into B, ending in a valve opening upwards into B.

Operation. When the water in the boiler gets low, the float in B falls; the uppermost pin on the float rod then presses upon that arm of the tumbler on which there is no weight, and brings it down till the weight fixed on the other arm of the tumbler, passing the vertical position, falls down on the other side towards the boiler, and striking against one of the pins in the round plate, which forms the head of the cock *g*, (and is coloured red *) by a sudden jerk opens it. The steam then rushes into E, which is previously full of water, and pressing on the surface thereof, forces it all through the branch K into B. This sudden accession of water in B raises the float, and in rising the lowermost pin on the float rod raises up the tumbler, by which the cock *g* is instantly shut. The steam in E being thus insulated soon becomes condensed, and feed water rushes up the feed pipe *i i* and fills E again ready for delivery into the boiler whenever the fall of the float solicits another supply. In order to keep *g* shut till the vacuum takes place, the register cock *d* is shut just so much as to allow the contents of B to pass thereout leisurely into the boiler. For this purpose the water way of the register cock may be more or less contracted by its handle, as found necessary; *l* is a weight suspended to a lever which counterpoises the float in B. The drawing, Fig. 9, represents the cock *g* having been newly opened,

* See note, page 325.

and

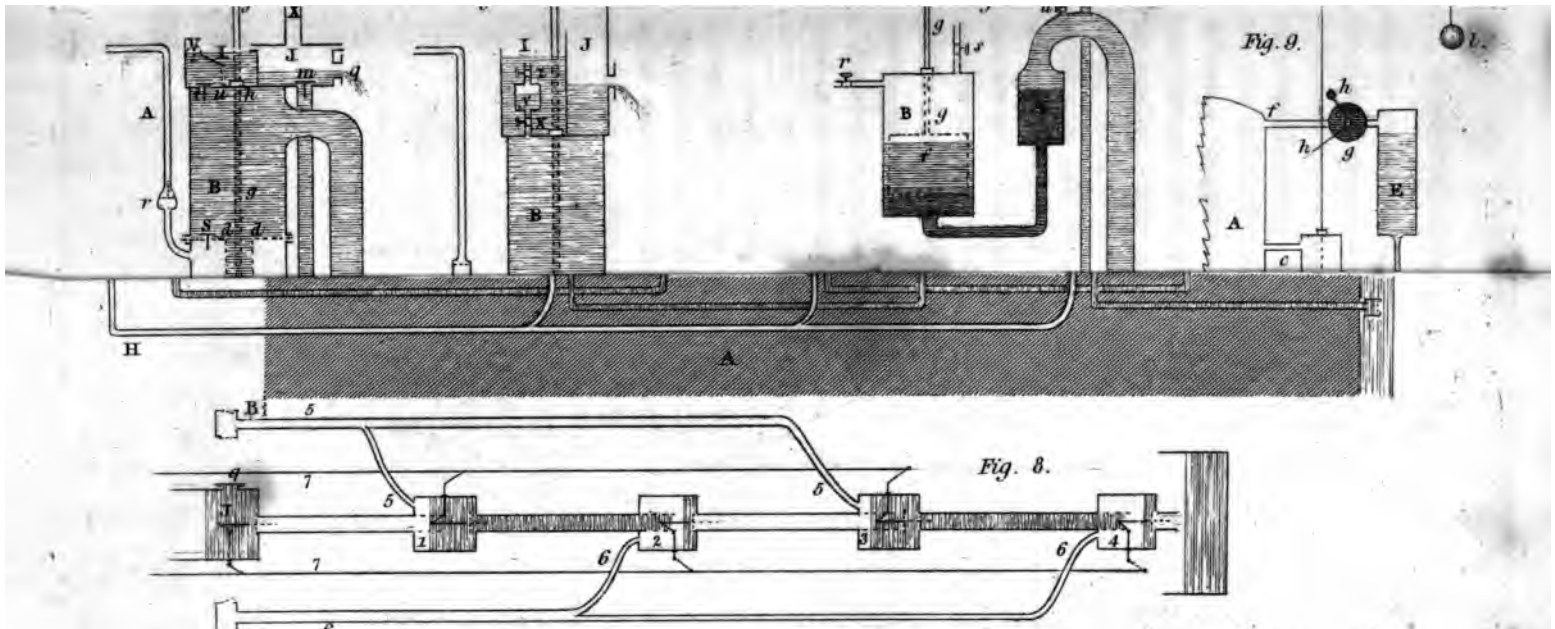


Fig. 9.
A
B
H
E
c
Fig. 8.
1
2
3
4
5
6
7

and the steam forcing the water out of E into B, which has already produced a temporary rise of the float in B.

Various modifications of this principle of feeding may be adopted, though different from this. Thus A, in Fig. 10, is a boiler; *b c* are two cocks on a pipe leading into A from *d* the feed cistern. If by the hand-gear *b* and *c* were opened alternately (one of them being always shut) the feeding would go regularly on. But whenever the surface of the water in the boiler was higher than the end of the pipe next *b*, although the cocks opened as usual, no water would go into A, because no steam could get into the pipe to displace the water. Instead of the cock *c* a valve opening towards the boiler may be used, which will work itself so that only the cock *b* will need turning at intervals.

General Observations. Either cocks or valves may be used in all these operations, and any fluid may be raised as well as water, or water whether pure or mixed with any other substance, and the alternate motion of the water by any of these modes may be made to work machinery by means of a piston, or otherwise, instead of raising other water; or otherwise the raised water may be made to operate as a first mover.

In witness whereof, &c.

Specification of the Patent granted to WILLIAM SCOTT, *of the London Glass-works, East Smithfield, in the County of Middlesex, Glass Manufacturer; for Improvements in the manufacturing and working of various Kinds of Glass.*

Dated August 9, 1805.

TO all to whom these presents shall come, &c. NOW KNOW YE, that in compliance with the said proviso, I the said Wiliam Scott do hereby describe and ascertain

tain the nature of my said invention, and the manner in which the same is to be performed, as follows; that is to say: The object of my patent is to grind and polish glass of various species, of a thinner substance than has hitherto been practised, and rendering such fit for window-sashes, mirrors, or looking-glasses, either from plate, brown or flint glass, by flashing or expanding such in the process of manufacturing, by a rotative motion, in a similar manner to window-glass, preserving such thickness in the blowing as may be deemed necessary. When drawn from the annealing kilns, to be cut into squares or panes, placing two, three, or more, upon each other in spreading or annealing kilns, upon flat surfaces of stones, glass, or other substances, and producing such degree of heat as will cause the glass to give or yield to the surface thus placed upon, thereby becoming flat and adapted for grinding, and polishing without grinding, rendering such fit for silvering; at which time they should be withdrawn.

Also to grind and polish sheet or spread glass, as has not been made, used, or intended for that purpose; namely, grinding and polishing, by flattening, as above described, and by buckling, tying, or fixing such to beds of plaster, for grinding, and for polishing.

The methods hitherto usually adopted for manufacturing plate-glass intended for such purposes, are by blowing cylinders, and cutting them open with shears at each end, or casting on metal tables; either of which processes necessarily requires a thickness or substance greatly exceeding the mode I have proposed.

In witness whereof, &c.

Specification

Specification of the Patent granted to SAMUEL ANNESS, *of Red-lion-place, in the Parish of St. Sepulchre, in the City of London, China-enameler; for Methods of preparing various Enamel Colours, and of applying the same so prepared to the ornamenting useful Vessels of Glass.* Dated Nov. 26, 1805.

TO all to whom these presents shall come, &c. Now KNOW YE, that in compliance with the said proviso, I the said Samuel Anness do hereby declare, that my said invention of certain improved methods of preparing various enamel colours, and of applying the same so prepared to the ornamenting useful vessels of glass, and the manner in which the same is to be performed, are described as follows; that is to say: the object, aim, and purpose in the composition of the said colours, beside their particular respective tints, consist in making them so fusible as to melt or adhere to vessels of glass by a degree of heat not so considerable as to melt or injure the vessels themselves; and the composition and modes of application of the said colours are as follows:

To prepare the Flux or principal Matter for Enamelling on Glass Vessels.

Method I. Take 1 lb. of saturnus glorificatus (to prepare glorificatus thus: take litharge or white lead, put it in a pan, pour on distilled vinegar, stirring it well over a gentle fire till the vinegar becomes impregnated with the salt of the lead, evaporate half vinegar, put it in a cool place to crystallize, and keep the crystals dry for use); half a pound of natural crystal calcined to a whiteness; one pound of salt of polverine, or other fit alkali; mix them together, and bake in a slow heat for about

about twelve hours, then melt the mass, and pulverize the same in an agate mortar, or any other proper vessel which is not capable of communicating any metallic or other impurity.

Method II. Or take three pounds of saturnus glorificatus; three quarters of a pound of natural crystal, as above; two pounds of salt of polverine, or more or less of any of the above; mix and beat the same, as in method I. always observing to put more crystal, if required to be harder and less fusible, and more salt of polverine if softer and more fusible; and farther, as all colours must be mixed with more or less of one of these or the following fluxes, those colours which abound most with metal or metals will require a less proportion of flux or fusible matter than those wherein there is much earth, stone, &c.

Method III. Take litharge, or white or red lead, or any preparation of lead or lead ores, and pearl-ash, nitre, borax, soda, or any salt, natural or prepared, which is capable of assisting the vitrification of stones, earths, sands, clays, spars, fossils, calcareous matters, metals, semi-metals, minerals, &c. and take sand, flints, or any stones, earths, clays, crystals, spars, fossils, or petrifications, glasses of any kind, whether transparent or opake; mix more or less of any or all of them together, as the hardness or softness of the flux requires. But I think Method I. the best.

To make Green.

Method I. Take one ounce of copper dust, two ounces of sand, one ounce of litharge, half an ounce of nitre; or two of copper, one of sand, two of litharge, one and a half of nitre; mix them with equal parts of flux,

flux, or vary the proportions of them, as may be found necessary, according to the tint of colour required.

Method II. Or take blue or green vitriol or azustam or any preparation of copper or copper ores, or cobalt, crude or prepared, or smalt or any blue made from lapis lazuli, or azure-stone or any other matter which will stand the test of fire, and antimony or any preparation of Naples yellow, mixed in due proportion with any kind of blue glass or enamels, and yellow glass or enamels; melt them together by fire, or use them raw, by simply grinding with a proportion of flux, as may be found most convenient, according to the glass it is used upon, as the glass of different manufactories differs much in hardness and properties; or, instead of sand, take pulverized flint, natural crystal, or any stone or earth that is vitrifiable; but I think sand best; and instead of litharge, take red lead or any preparation of lead, and instead of common nitre, take soda, rochettes, borax, or any alkaline, or any other salt, natural or factitious, or bone-ash or any other ash: any of these or any fit material which will assist the vitrification of stones, earths, glass, &c. Take any proportion of any or all of the above, according to the tint of the colour required: but I think Method I. much the best.

To make Black.

Method I. Take calcined iron one ounce, cobalt, crude or prepared, one ounce, or zaffer two ounces, and manganeze one ounce; mixed with equal parts of flux, by melting or grinding together.

Method II. Take sand, pulverized flint, flint-glass, or any stone or earth, or vitrifiable substance; mix them with blue or green vitriol, calcined copper, or any preparation of copper or copper ores, gold, antimony, or

any metal, ore, earth, calcareous matter, natural or prepared, containing any of the aforesaid metals, &c. temper them with enamels, or glasses of any kind, according to the tint or quality required; they may be rendered softer with any of the preparations of lead or salts mentioned in the method for making green, and harder by the addition of more of the tinting matter: but Method I. is much the best.

To make Yellow.

Method I. Take of lead and tin ashes one ounce, litharge one ounce, antimony one ounce, sand one ounce, nitre four ounces; calcine or melt them together, pulverize, and mix them with a due proportion of flux, as the nature of the glass may require; or take more or less of any or all the above, according to the depth of colour desired.

Method II. Take any preparation of Naples yellow, or silver, or any compound which contains silver, or antimony, or any metal, semi-metal, mineral, clay, earth, or stone, or calcareous matter which will yield a yellow, that will stand the test of fire; any or all may be tempered with any kind of yellow enamel or yellow glass, or made harder or softer by the addition of any of the salts, leads, flints, earths, or stones, &c. mentioned in the method to make green; always observing, that the leads, salts, &c. render it softer, and the flints, earths, stones, &c. harder, as is the case with all enamels and glasses: but I think Method I. the best.

To make Blue.

Method I. Take prepared cobalt one ounce, sand one ounce, red lead one ounce, nitre one ounce, flint glass two ounces; melted together by fire, pulverized and fluxed

fluxed according to the degree of softness or strength of colour required.

Method II. Take smalt of any kind, or blue prepared from lapis lazuli or azure-stone, or copper or silver, or any kind of metal, semi-metal, mineral, earth, or calcareous matter, from which a blue or blue-green can be drawn which will stand the test of fire; mix them with flux, or any kind of glass or enamel, and render it more hard, with an addition of pulverized flint, or stone, or earth, or any vitrifiable substance; or more soft, by any preparation of lead, or any kind of salt, natural or prepared: but I think Method I. the best.

To make Olive.

Method I. Take one ounce of the blue as prepared in Method I. half an ounce of black as prepared in Method I. half an ounce of yellow as prepared in Method I.; grind them for use; if necessary, add flux to make it softer.

Method II. Take of any of the blues, or any or all the materials from which the blues are made, any of the black, or any or all of the materials from which the blacks are made, or any of the browns, or any or all of the materials from which the browns are made, and yellow, or any or all of the materials from which the yellows are made; mix any or all these together as the tint requires; harden or soften them as is observed for the greens, or take any metals, semi-metals, minerals, ores, flints, sands, earths, stones, spars, petrifactions, or any calcareous matter, or any matter which will yield a colour when fluxed: but the first is the best.

To make White.

Method I. Take tin prepared by aqua-fortis one ounce, red lead one ounce, of white pebble-stone or natural

tural crystal two ounces, nitre one ounce, arsenic one drachm, with equal parts of flux, or more or less as the softness or opacity may require; melted, calcined, or used raw.

Method II. Take lead and tin ashes, or any preparation of lead and tin, and bone-ash, or any matter containing arsenic, or any white stone, earth, or clay, or any calcareous matter, or any wares or porcelains, talcs, spars, plaster of Paris, chalk, lime, or whitening, or any matter which will retain a whiteness in the fire, or any white glass, or any white enamel; take any or all of these, and temper them with any of the preparations of lead or salt, or both, to render them more soft, as mentioned in method to make green; or harden them with sands, flints, stones, clays, earths, &c. more or less of any or all: but I think Method I. the best.

To make Purple.

Method I. Take the finest gold, dissolve it in aqua-regia, regulated with sal ammoniac; put it in a sand heat for about forty-eight hours to digest the gold, collect the powder, grind it with six times its weight of sulphur, put it into a crucible on the fire till the sulphur is evaporated, then amalgamate the powder with twice its weight of mercury, put it into a mortar or other vessel, and rub it together for about six hours, with a small quantity of water in the mortar, which change frequently, evaporate the remaining mercury in a crucible, and add to the powder ten times its weight of flux, or more or less as the hardness or softness of the colour may require.

Method II. Dissolve gold in aqua-regia, precipitate it with a solution of tin in aqua-fortis; mix it with eight

times

times its weight of flux, or more or less as the hardness or softness of the colour may require.

Method III. Or take manganese and copper, and any preparation of cobalt, with salt of tartar, temper them with any kind of purple, blue, or black enamels, or glasses, or ingredients for making enamels or glasses; take more or less of any or all these, and flux it or them as the colour requires. The gold may be prepared by amalgamation only, or solution only, or by any precipitate, evaporation, calcination, or fusion: but I think Method I. preferable.

To make Rose Colour.

Method I. Take purple as prepared Method I. mix it with thirty times its weight of flux, and one hundredth part of its weight of silver leaf, or any preparation of silver, or vary the proportion of the flux and silver, as the quality of the colour may require; or any of the other preparations for purple will do, varying the proportion of the flux and silver as above; or any materials, from which purple can be produced, will with the addition of silver and flux, answer: but Method I. is the best.

To make Brown.

Method I. Take red lead one ounce, calcined iron one ounce, antimony two ounces, litharge two ounces, zaffer one ounce, sand two ounces, calcined, or melted together, or used raw, as may be most expedient, or vary the proportions of any or all the above as tint or quality may require.

Method II. Take any preparation of antimony, cobalt, manganeze, tin, silver, gold, iron, lead, copper, zinc, platina, nickel, arsenic, bismuth, mercury, or any other metal, semi-metal, mineral, clay, earth, stone, sand, or any calcareous matter, natural or prepared, from which colour

colour can be drawn, and temper any or all of the above with flux, or any enamel or ingredient for making enamel or glass, or ingredients for making glass. Browns may be produced in greater abundance from a greater variety of metals, semi-metals, minerals, earths, ochres, umbers, and calcareous matters, than any other colour, as almost every metal will yield a brown: but I think Method I. the best.

Method of application. The aforesaid colours may be applied to vessels of glass in the following manner: *viz.* by painting, printing or transferring, dipping, floating, and grounding.

To paint, mix the colours (when reduced by grinding to a fine powder) with spirits of turpentine, temper them with thick oil of turpentine, and apply them with camel hair pencils, or any other thing thought proper; or mix them with nut or spike oil, or any other essential or volatile oil, or with water, in which case use gum arabic, or any other gum that will dissolve in water, or with spirits, varnishes, gums of any kind, waxes, or resins: but the first I conceive to be the best.

To print, take a glue bat, full size, for the subject, charge the copper-plate with the oil or colour, and take the impression with the bat from the plate, which impression transfer on the glass; if the impression be not strong enough, shake some dry colour on it, which will adhere to the moist colour; or take any engraving or etching, or stamp, or cast, and, having charged it with the oil or colour, transfer it on the glass by means of prepared paper, vellum, leather, or any other substance that will answer: but I think the first the best. Any of the aforesaid engravings, etchings, stamps, casts, or device, may be charged with waters, oils, varnishes, or glutinous matters

matters of any kind, reduced to a proper state, as is necessary in printing in general; any or all of these may be used alone, or mixed with the colours. When used alone, the colour is to be applied in powder as before mentioned.

To dip, mix the colour to about the consistency of a cream with any of the ingredients used for printing, wherein dip the glass vessel, keep it in motion till smooth.

To ground, first charge the glass vessel with oil of turpentine, with a camel-hair pencil, and while moist apply the colour in a dry powder, which will adhere to the oil, or, instead of oil of turpentine, use any of the materials used for printing: but I think the first the best.

To float, mix the colour with any of the ingredients used for printing, to a consistency according to the strength of ground required, float it through a tube, or any other vessel, moving or shaking the piece of glass till the colour is spread over the part required.

In witness whereof, &c.

Specification of the Patent granted to MARC ISAMBARD BRUNEL, *of Portsea, in the County of Hants, Gentleman; for Saws and Machinery upon an improved Construction for sawing Timber in an easy and expeditious Manner.* Dated May 7, 1805.

With a Plate.

TO all to whom these presents shall come, &c. NOW KNOW YE, that in compliance with the said proviso, I the said Marc Isambard Brunel, do hereby declare, that my said invention is described by the drawings and explanations thereof hereunto annexed, and the following description

scription thereof, *viz.* the saws are intended to be of a circular form; and, in order to obtain a great diameter, made of two or more pieces of sheet steel, properly adjusted and fixed together, see Figs. 1, 2, 3, 4 and 7, (Plate XIV). The saw Fig. 1, is made of eight pieces. The pieces A A A having been cut to the proper shape, and fitted together at the edges, as represented at B, Fig. 4, are screwed (see 1, 1, 1, Fig. 1.) against a flanch A, Fig. 2, which has been previously turned very flat; the holes through which the screws pass are cut in an oblong form, in order to admit of adjustment. When the plates have been thus fastened to the flanch A, another flanch C, Fig. 1 and 2, is laid upon the plates; and, in order to make it fit, and bear with equal power upon each plate, several thicknesses of paper or leather, of the size of the flanch, are placed between it and the plates. The flanch C, is fastened to the other by screws 2, 2, 2, &c. Before they are screwed tight the plates are drawn in concentrically, by means of wedges 3, 3, 3, &c. in order to close the joints. It must be observed that the joints made in the form B, Fig. 4, must be turned according to the direction of the revolving motion of the saw. Saws composed of three or more pieces are intended to be fitted and fastened in the manner before described; if composed of two pieces (see Fig. 3) the joining edge must be hollow at the part A of the plate, and sharp at B, see *o* A and *o* B.

Specification of the Machinery. The Improvements in the machinery for sawing timber in an easy and expeditious manner consist in the modes of laying and holding the piece of wood in the carriage or drag, in the facility of shifting the saw from one cut to another, and in the practicability of sawing both ways either towards or from the saw or saws.

The

The Figures 5, 6, 7, and 8 *, represent the machinery fitted up with one circular saw only. The circular saw A is adjusted upon a spindle, of a cylindrical form, which turns within rodings C C. The saw is intended to be turned with either a strap or a band, moved by any power (wind, water, steam, horses or men). The drum D Figs. 5 and 6, is to receive a strap. The log or piece of timber (see E, Figs. 5, 6, 7, 8,) is placed upon a drag or carriage F, and held fast by means of clamps G. The carriage or drag is moved to and from the saw by the handle or crank H, Figs. 5, 6, and 8, communicating, by the assistance of cog wheels, to a pinion J, Figs. 5 and 7, which engages in a rack K, Fig. 7. The drag or carriage is furnished with rollers, in order to ease its longitudinal motion, and is intended to be moved by hand, in order to accelerate or stop it at pleasure. The length of the carriage or drag is according to the size of the timber intended to be converted by such machinery.

When the saw has performed one cut, it is shifted to the next in the following manner: supposing, for example, that the saw has been through the first cut 1, see the log E, Fig. 5, the saw is then moved collaterally to the next cut 2, and so on, and kept to its place by means of a screw N, Figs. 5 and 6. This screw N is complete at one end to the extremity of the cylindrical shafts, and passes in a direction parallel to it through the rodings C +, Figs 5, 6, and 8. This manner of sawing timber requires no fastening to the log when it is intended to be slabbed, only excepting, however, when the log is crooked, in which case it may be forced to become straighter by the assistance of cramps G, G, Figs. 5, 6, 7, 8. M, M, Fig.

* Fig. 5 is a transversal section, Fig. 6 a plan, Fig. 7 a lateral section, Fig. 8 an elevation.

8, represent circular wedges intended to follow the cut opened by the saw, and by that means to ease the friction, and to steady the piece of wood. The circular wedges move collaterally when shifted, in order to meet with the next cut of the saw. The drag or carriage might be moved (if found more advantageous) by the machinery, which gives motion to the saw.

The Figs. 9 and 10 represent the manner of adjusting several saws on one spindle, by which means a piece of wood may be partly or entirely converted at one operation. In that case the flanches of the saws are fixed upon an iron drum A, Figs. 9 and 10, and kept firm and close with each other by four bolts 1, 2, 3, 4, Fig. 10. In order to lower the saw or saws when they wear away, the side rails P, P, Figs. 9 and 10 may be depressed by means of the wedges Q, Q, Figs. 9 and 10. It must be observed, that the log or piece of wood does not lay close upon the drag or carriage; it is raised by some pieces placed crosswise, see O, O, O, Figs. 5, 6, 7, and 8: the circular wedges described at M M, Fig. 8, are also represented at M. Fig. 11, they revolve by the motion of the log, and keep each piece, and consequently the whole log, steady.

Fig. 12 represents an instrument composed with one or several plates of metal to be used in lieu of the circular wedge or wedges. The distance between these plates is regulated by that between the saws.

Fig. 13, represents the breadth of the plates described above.

In witness whereof, &c.

Note. There are several letters marked in the drawing attached to the specification enrolled in the office which are not mentioned in the description, they have therefore been omitted in the plate.

Additional

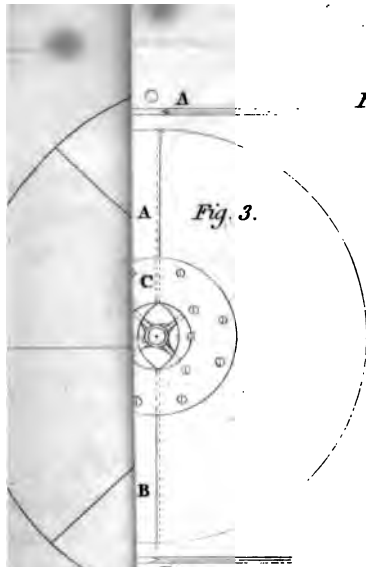


Fig. 3.

Fig. 4.

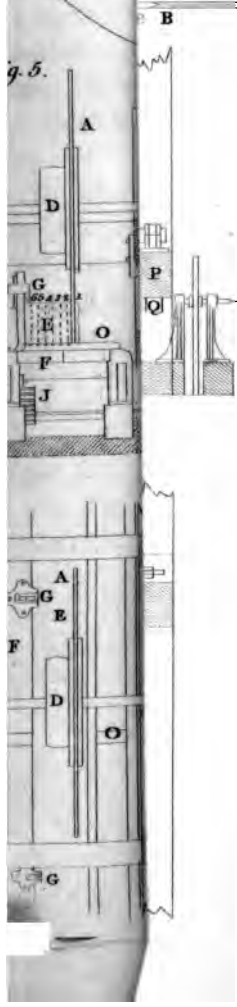


Fig. 10.

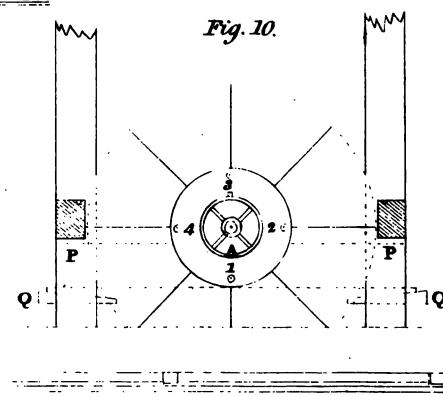


Fig. 13.

Fig. 12.

Additional Experiments and Remarks on an artificial Substance, which possesses the principal characteristic Properties of Tannin.

By CHARLES HATCHETT, *Esq. F. R. S.*

(Concluded from Page 362.)

§ V.

AS many vegetable substances when roasted, yield by decoction a liquid which in appearance much resembles the artificial tanning matter when dissolved in water; I roasted some of the common dried peas, horse-beans, barley, and wheat flour, the decoctions of which however did not afford any precipitate by solution of isinglass.

Even the decoction of coffee did not yield any precipitate by this method, until several hours had elapsed, and I found that the precipitate so formed was permanently soluble in boiling water. But to explain this, we must recollect, it is extremely probable, that some peculiar nicety is required in the roasting of such bodies before the tanning substance can be developed: and this seems to be corroborated by some experiments which I made on the decoction of a sort of coffee prepared from the chicorée (I suppose endive) root, which was given me by Sir JOSEPH BANKS; for although this decoction did not afford an immediate precipitate with solution of gelatine, and although the precipitate was also apparently dissolved by boiling water, yet upon cooling, the same precipitate was reproduced in its original state. I am therefore inclined to believe, that the tanning substance is really developed in many of the vegetable bodies by heat, but that a certain degree of temperature, not very easy to determine, is absolutely requisite for this purpose.

Before I conclude this section, it may be proper to observe, that when a small quantity of nitric acid was added to any of the above-mentioned decoctions, and when these had been subsequently evaporated to dryness, and afterwards dissolved in distilled water, they were converted into a tanning substance perfectly similar to that which is produced by the action of nitric acid on the varieties of coal.

§ VI.

In the preceding Paper, a variety of the tanning substance was slightly noticed, which was formed by the action of sulphuric acid upon common resin, elemi, amber, &c. &c.; and as an instance has occurred of the formation of the same substance from camphor, accompanied by circumstances which tend to increase our knowledge of the properties of the latter, I shall here describe this experiment.

Experiment on Camphor with sulphuric Acid.

The effects produced on camphor by sulphuric acid have been but very superficially examined; for all that has hitherto been stated amounts to this, that camphor is dissolved by sulphuric acid, that a brown or reddish-brown solution is formed, and that the camphor is precipitated unchanged from this solution by water. These facts, however, only relate to a certain period of the operation, for if this be long continued, other effects are produced, which I shall now describe.

A. To one hundred grains of pure camphor put into a small glass alembic, one ounce of concentrated sulphuric acid was added. The camphor immediately became yellow, and gradually dissolved, during which, the acid progressively changed to brownish-red, and afterwards to brown. At this period, scarcely any sulphureous acid

was

was evolved, but in about one hour the liquid became blackish-brown; much sulphureous acid gas was then produced, and continued to increase during four hours, when the whole appeared like a thick black liquid, at which period not any odour or appearance of camphor could be perceived, but only that of the sulphureous acid. After two days, during which time the alembic had not been heated, there did not appear any alteration, unless that the production of sulphureous gas was much diminished. The alembic was then placed in a sand-bath moderately warm, by which more of the sulphureous gas was obtained, but this also soon began to abate. After the lapse of two other days, I added gradually six ounces of cold water, by which the liquid was changed to reddish-brown, a considerable coagulum of the same colour subsided, the odour of sulphureous gas, which in some measure had still prevailed, was immediately annulled, and was succeeded by one which resembled a mixture of oils of lavender and peppermint.

The whole was then subjected to gradual distillation, during which, the water came over strongly impregnated with the odour above-mentioned, accompanied by a yellowish oil which floated on the top of it, and which, as far as could be ascertained, amounted to about three grains.

B. When the whole of the water was come over, there was again a slight production of sulphureous gas. I then added two ounces of water, which I drew off by distillation, but did not obtain any of the vegetable essential oil which has been mentioned, nor did the odour of it return. I therefore continued the distillation until a dry blackish brown mass remained; this was well washed with warm distilled water, by which, however, nothing was extracted; but when two ounces of alcohol were digested on

it

it during twenty-four hours, a very dark brown tincture was formed.

The residuum was digested with two other ounces of alcohol in like manner, and the process was repeated until the alcohol ceased to act.

The residuum had now the appearance of a compact sort of coal in small fragments; it was then well dried, and after exposure to a low red heat in a close vessel weighed fifty three grains.

C. The different portions of the solution formed by alcohol were added together, and being distilled by means of a water-bath, a blackish brown substance was obtained, which had the appearance of a resin or gum with a slight odour of caromel, and weighed 49 grains.

The products therefore which were thus obtained from 100 grains of camphor when treated with sulphuric acid, were,

	Grains,
A. An essential oil which had an odour somewhat resembling a mixture of lavender and peppermint, about	3
B. A compact and very hard sort of coal in small fragments	53
C. And a blackish brown substance of a resinous appearance	49
	105

From this statement it appears, that there was an increase in the weight amounting to five grains, which I attribute partly to oxygen united to the carbon, and partly to a portion of water so intimately combined with the last product, that it could not be expelled from it by heat without

without subjecting it to decomposition. The properties of this substance were as follows:

1. It was extremely brittle, had somewhat of the odour of caromel, the flavour was astringent, and it speedily dissolved in cold water, and formed with it a permanent dark brown solution.

2. This solution yielded very dark brown precipitates by the addition of sulphate of iron, acetite of lead, muriate of tin, and nitrate of lime.

3. Gold was copiously precipitated by it from its solution in the metallic state; and

4. By solution of isinglass, the whole was completely precipitated, so that after three or four hours, a colourless water only remained.

The precipitate was nearly black, and was insoluble in boiling water; from which property, as well as from the effect produced upon prepared skin by the solution, it was evident, that the substance thus obtained from camphor, was a variety of the artificial tanning matter, much resembling that which may be obtained from resinous bodies by means of sulphuric acid. But it must be observed, that this sort of tanning substance seems to act less powerfully on skin, than that which is prepared from carbonaceous substances by nitric acid, and the precipitate which the former produces with solution of gelatine is more flocculent and less tenacious, than that which in like manner is formed by the latter.

It is however remarkable, that when a small quantity of nitric acid was added to the solution of the substance obtained from camphor, and when after evaporating it to dryness, the residuum was dissolved in water, a reddish brown liquid was formed, which acted in every respect similar to the tanning substance obtained from the varieties of coal by nitric acid.

§ VII.

§ VII.

From the experiments which have been related, it appears that three varieties of the artificial tanning substance may be formed, viz.

1st. That which is produced by the action of nitric acid upon any carbonaceous substance, whether vegetable, animal, or mineral.

2dly. That, which is formed by distilling nitric acid from common resin, indigo, dragon's blood, and various other substances; and,

3dly. That which is yielded to alcohol by common resin, elemi, asa fœtida, camphor, &c. after these bodies have been for some time previously digested with sulphuric acid.

Upon these three products I shall now make a few remarks, which I have hitherto postponed, in order that the account of the experiments might not be interrupted.

The first variety is that which is the most easily formed; and from some experiments which were purposely made, I find that 100 grains of dry vegetable charcoal afford 120 of the tanning substance; but as it is extremely difficult completely to expel moisture, or even the whole of the nitric acid which has been employed*, an allowance of about three or four grains ought to be made, so that after this deduction we may conclude, that 100 grains of vegetable charcoal yield 116 or 117 of the dry tanning substance.

The proportions of the constituent parts of this substance I have not as yet ascertained; but from the manner by which it is produced, carbon is evidently

* The most effectual method of expelling the nitric acid, is to reduce the tanning subtance to powder, and repeatedly evaporate different portions of distilled water from it in a glass or porcelain basin.

the

the base of it, and is the predominating essential ingredient.

From § III. experiment F, it also appears, that the other component parts are oxygen, hydrogen, and nitrogen; for when the artificial tanning substance was distilled, ammonia and carbonic acid were obtained, exclusive of a very small portion of a yellow liquor, which stained the upper part of the retort, and which, from its tenacity and insolubility in water and alcohol, appeared to be of an oily nature.

As I had taken every precaution respecting the charcoal which had been employed, I was at first induced to consider the above facts as almost positively demonstrative of the presence of hydrogen in charcoal, but upon farther reflexion, and upon weighing some of the circumstances which attend the formation of the artificial tanning substance, I still feel on this point very considerable doubt; for I have constantly observed, that diluted nitric acid, acts upon charcoal more effectually, in the formation of the tanning substance, than when it is employed in a concentrated state; and it appears therefore very probable, that hydrogen may have been afforded by a portion of water decomposed during the process. For admitting that the new compound (formed by the action of nitric acid upon charcoal) may possess a certain degree of affinity for hydrogen, this being exerted simultaneously with the affinity for oxygen possessed by nitrous gas, may (especially when the last is in a nascent state) effect a decomposition of a portion of water, the hydrogen of which would therefore enter into the composition of the tanning substance, whilst the oxygen would supply the place of part of that which had been taken from the nitric acid.

Many of the properties of the tanning substance prepared from coal by nitric acid are very remarkable, par-

ticularly those which have been noticed in § III. experiments F and G.; for surely it is not a little singular, that this substance when burned should emit an odour so very similar to animal matter, notwithstanding that the tanning substance had been prepared from pure vegetable charcoal. And again in experiment G. the portion which had not been precipitated by solution of isinglass, was, when dried, found to possess a strong vegetable odour very analogous to oak bark, although charcoal is inodorous, and isinglass very nearly so.

But, after all, the most extraordinary properties of this substance are certainly those which so nearly approach it to the vegetable principle called *tannin*; for it perfectly resembles this principle by its solubility in water and in alcohol, by its action upon gelatine and upon skin, by the effects which it produces upon metallic solutions, upon those of the earths, and of the alkalis.

The sulphuric and muriatic acids also affect the solutions of it as they do those of *tannin*; and the only marked difference which as yet has been found in the characters of the artificial substance and of *tannin*, is, that the former is produced, whilst the varieties of the latter are more or less destroyed by nitric acid. This, for the present at least, must draw a line of separation between them, but we must not forget, that even the varieties of *tannin* * do not accord in the degree of destructibility.

The

* I shall here venture to state some ideas which have occurred to me on the probable cause and mode of the formation of tannin.

Mr. Biggin has proved, that similar barks, when taken from trees at different seasons, differ as to the quantities of tannin contained in them. (Phil. Trans. 1799, p. 259.)

Mr. Davy also observes, "that the proportions of the astringent principles in barks vary considerably according as their age and size are different."

"That

The second variety of the tanning substance is obtained from a great number of vegetable bodies, such as indigo, dragon's

"That in every astringent bark the interior white bark (which is the part next to the alburnum) contains the largest quantity of tannin. The proportion of extractive matter is generally greatest in the middle or coloured part; but the epidermis seldom furnishes either tannin or extractive matter."

Moreover Mr. Davy remarks, "that the white cortical layers are comparatively most abundant in young trees, and hence their barks contain in the same weight a larger proportion of tannin than the barks of old trees." Phil. Trans. 1803, p. 264.

We find, therefore,

1st. That the proportion of tannin in the same trees is different at different seasons.

2dly. That tannin is principally contained in the white cortical layers, or interior white bark which is next to the alburnum or new wood: and

3dly. That these white cortical layers are comparatively most abundant in young trees, and that their barks consequently contain in the same weight more tannin than the barks of old trees.

I shall not make any remarks on the first of these facts, as it accords with other similar effects, which are the natural consequences of the processes and periods of vegetation; but the second and third appear to be important; for they prove that tannin is principally formed, or at least deposited, in the interior white bark, which is next to the alburnum or new wood; so that in the very same part where the successive portions of new wood are to be elaborated and deposited, we find the principal portion of tannin.

It should seem, therefore, that there is an intimate connexion between the formation of new wood and the formation of tannin in such vegetables as afford the latter; and this idea is corroborated when the chemical nature of these substances is considered.

From experiments made on the ligneous substance of vegetables, or the woody fibre, it appears to be composed of carbon, oxygen, hydrogen, and nitrogen, but of these its principal and essential ingredient is carbon.

In like manner carbon is unquestionably the basis and principal ingredient of tannin. Considering, therefore, that both of these sub-

dragon's blood, common resin, &c. &c. by digesting and distilling them with nitric acid. It is not, therefore, quite so readily prepared as that which was first described, and its relative quantity, when compared with that of the substance employed to produce it, is less.

As resin and some of the other bodies do not afford it until they have been repeatedly treated with nitric acid, and as during each operation nitrous gas is produced, whilst the strength of the acid which comes over is diminished, it seems almost evident, that this tanning substance is formed in consequence of part of the oxygen of the nitric acid becoming combined with the hydrogen of the original body, so as to form water; and the carbon being

stances consist principally of carbon, that tannin is secreted in that part of barks where the formation and deposition of new wood take place, and that the quantity of tannin is the most considerable in young trees, and seem therefore to keep pace with their more vigorous growth and consequent formation of wood, it appears very probable that those vegetables which contain tannin, have the faculty of absorbing more carbon and of the other principles than are immediately required in the formation of the different proximate vegetable substances, especially the woody fibre; that this excess, by chemical combination, becomes tannin, which is secreted in the white interior bark: that in this state it is a principle peculiarly fitted to concur by assimilation to form new wood: that it is therefore subsequently decomposed at the proper period, and is employed in the formation of the new wood: that there is not a continual accumulation of tannin in the vegetables which afford it, as it is successively formed in and with the white cortical layers, and is successively decomposed by concurring to form new wood; and lastly, that as the vegetable approaches more nearly to the full maturity of its growth, when wood is less rapidly and less plentifully formed, so in like manner less tannin is secreted, for the fabric being nearly completed, fewer materials are required.

Such I am inclined to suspect, from the facts which have been adduced, to be the cause and mode by which tannin is formed in oaks and other vegetables; but I make this statement only as a probable conjecture, which may be refuted or confirmed by future observations.

thus

thus in some measure denuded, is rendered capable of being gradually acted upon by the nitric acid in a manner nearly similar to that, which takes place when it has been previously converted into coal.

The colour of the precipitates which this tanning substance yields with gelatine is constantly pale or deep yellow, whilst that of the precipitates formed by the first variety is always brown; I am therefore induced to believe, that the different colours of the precipitates produced by the varieties of tannin depend on the state of their carbon.

When resin and the other bodies were treated as above described with nitric acid, the quantity obtained of the tanning substance was much less than when an equal quantity of coal was employed, or even when these bodies had been previously converted into coal in the humid way by sulphuric acid.

The cause of this seems to be, that a number of other products are simultaneously formed, all of which require more or less of carbon as a constituent ingredient, so that, in consequence of the affinities which prevail under the existing circumstances, some bodies by treatment with nitric acid afford but little, and others none of the tanning substance.

The greatest proportion of this substance was yielded by indigo, common resin, and stick lac.

The quantity obtained from asa fœtida and gum ammoniac was less.

Benzoin, balsam of Tolu, balsam of Peru, and dragon's blood, were inferior to the former in this respect, so that the developement or rather production of benzoic acid * appeared partly to counteract the formation of the tanning

* The expression, "*production of benzoic acid*," may appear objectionable, and I shall therefore take this opportunity to observe, that I much

taining substance. But oxalic acid when formed in any considerable

much suspect the present established opinion respecting the balsams and benzoic acid to be erroneous. For the balsams are defined to be dies composed of resin and benzoic acid; consequently the latter, when obtained in a separate state, is considered as an original ingredient or educt.

I am however inclined to a contrary opinion, for I consider the balsams as peculiar substances, which, although nearly approaching to the nature of resins, are nevertheless different in respect to the original combination of their elementary principles, which combination however is with much facility modified by various causes, and especially by a certain increase of temperature, so that a new arrangement of the elementary principles takes place, part being formed into resin, and part into benzoic acid.

Many facts appear more or less to support this opinion; for whether benzoic acid is obtained by simple sublimation, or by merely digesting benzoin in boiling water, according to Geoffroy's method, or by the addition of lime, as recommended by Scheele, or by employing alkalis in a similar manner, nothing positive can be inferred from any of these operations to prove that benzoic acid is obtained as an educt, but rather the contrary, when we reflect on the affinities which are most likely to prevail under the circumstances of the different processes, and on the variable proportions of the benzoic acid; and although benzoic acid has been discovered in the urine of infants, in that of many adults, and constantly in that of graminivorous quadrupeds, such as the camel, the horse, and the cow, (*Système des Connoissances Chimiques*, par Fourcroy, 4to edit. Tome IV. p. 158;) yet all this certainly appears to be in favour of its being a chemical product.

I have observed, when benzoin, balsam of Tolu, and balsam of Peru, were dissolved in sulphuric acid, that a great quantity of beautifully crystallized white benzoic acid was sublimed during digestion; and as it is produced in so very pure a state by this single and simple operation, I would recommend a trial of the process to those who prepare benzoic acid for commerce; but I am not certain whether this mode may prove more economical than those which at present are employed.

When dragon's blood, however, was treated in the same manner with sulphuric acid, I could not obtain a particle of benzoic acid; nor

did

considerable quantity, seemed *absolutely to prevent* the formation of this substance; for whilst abundance of the former was obtained from gum arabic, tragacanth, manna, and guaiacum, not any of the latter could be produced.

Common liquorice appears at first to be an exception, but from the smallness of the quantity and the colour of the precipitate which it produced with solution of isinglass, I am almost convinced that the tanning substance was formed by the action of the nitric acid on a portion of uncombined carbon, which being in a state approaching to coal, is probably the cause of the blackness of the common liquorice.

As the formation of the tanning substance has been my principal object, I have not thought it necessary to enter at present into too minute a detail of other particulars, and have therefore only thus cursorily noticed some of the

did I succeed much better when I had recourse to lime, according to Scheele's process; for although a considerable quantity of the substance was thus rendered soluble in water, yet by the addition of muriatic acid I obtained only a slight appearance of benzoic acid accompanied by a copious precipitate of red resin, notwithstanding that the solution had acquired a powerful and peculiar balsamic odour.

But in a former part of this Paper I have stated, that when dragon's blood was dissolved in nitric acid, and afterwards evaporated to dryness, it yielded about 6 *per cent* of benzoic acid. Now if this had been originally present in dragon's blood in the state of benzoic acid, some stronger evidence of it might reasonably have been expected in each process; but this not being the case, I am inclined to consider it as produced, and not educed, by the action of the nitric acid on the original principles of the dragon's blood; and I am also persuaded that similar but more general effects take place when benzoin or any of the balsams are subjected to the different processes by which benzoic acid is obtained; so that to me, this last seems to be as much a chemical product, as the oxalic, the acetous, and other of the vegetable acids.

The succinic acid also appears to be a product and not an original ingredient of amber.

principal

principal effects produced by nitric acid on the resins, balsams, &c. Those however who are conversant with chemistry, will undoubtedly perceive that these effects deserve to be accurately investigated, and that the resins, balsams, gum resins, and gums, should be regularly examined by every possible method, not merely on account of the individual substances which may become the subjects of experiment, but because there is reason to expect that from such an investigation, medicine, with the arts, and manufactures, may derive many advantages, whilst the mysterious processes and effects of vegetation may very probably receive considerable elucidation.

Concerning the third variety of the tanning substance, which is produced by the action of sulphuric acid on the resins, &c. I shall here add but little to that which I have already stated in the latter part of the second section of my first paper, and in the account which I have lately given of an experiment on camphor.

This variety appears to be uniformly produced during a certain period of the process, but by a long continuance of the digestion, I have reason to believe that it is destroyed.

Substances, such as the gums, which afford much oxalic acid by treatment with other acids, do not apparently yield any of this tanning substance.

The energy of its action on gelatine and skin is certainly inferior to that of the first variety, into which however (as we have seen) it may easily be converted by nitric acid.

From the mode of its formation, there does not appear to be any positive evidence that it contains nitrogen like the first and second varieties, and perhaps the absence of nitrogen may be the cause of its less powerful action; this I have not as yet ascertained, but it is my intention more particularly to notice in a future Paper the general properties of this substance.

On

On the Use of Tobacco-water, in preserving Fruit Crops, by destroying Insects; and on the Use of the Striped or Ribband Grass, as Food for Cattle.

By Mr. ROBERT HALLETT, *of Axminster.*

From LETTERS and PAPERS of the BATH and WEST of ENGLAND SOCIETY.

BEING much engaged in mercantile concerns, and having but little time for other pursuits, I have not an opportunity, though extremely fond of my garden, of bestowing the attention to it I could wish: but having made a few experiments with a view of improving the state and bearing of my fruit-trees, in which I have succeeded beyond my highest expectations, I hope, as my intention is to benefit the public, I shall be pardoned for troubling you with the present communication of them.

The old gardeners with us have long entertained an idea that our climate has suffered a change particularly inimical to the successful cultivation of wall fruit-trees. To this circumstance they attribute the blight, which annually disappoints their hopes, and consider the evil beyond the reach of their skill to remedy.

The disease to which I have paid regard, is that which affects the trees in the early part of the season, curls up their leaves, often destroys the young shoot, and not unfrequently reduces the tree to a state of weakness, from which it is seldom to be recovered. I have, however, for some years past, successfully combated this baneful complaint, with a preparation easily to be obtained, attended with little expence, and yet certain in its effect.

The efficacy of tobacco in destroying insects has no doubt been long known, and which I was well aware of. But as the expence attending its use, either for fumigating

my trees, or washing them with an infusion, was considerable, and was perhaps the obstacle to its being generally resorted to, I endeavoured to find out the best method of obtaining it in quantities at a cheap and easy rate, of applying it with the least possible waste, and of preparing it so as to be used with safety. On considering the subject, it struck me that the tobacco-water used by shepherds, having the power of curing the scab in sheep, might answer my purpose; and having a tobacco and snuff manufacturer very near us, I applied to him, and had the pleasure of finding, that in pressing his tobacco he obtained large quantities of it, which he threw away as useless, except some little which he sold to shepherds. This liquor was exceedingly strong; and, after various trials, I found that a quarter of a pint, or indeed less, if it was tolerably thick, would impregnate a gallon of water with sufficient power to destroy every insect or reptile that felt its influence; and that two gallons of it, when diluted, were enough to wash all my trees, which are about fifty, three times over, and to preserve them throughout the season in the finest health and vigour.

My method of using it has been thus; as soon in the spring as I observe a leaf on any of my trees begin to curl, or be in the least diseased, I prepare my tobacco-water as I have before mentioned, *viz.* to something more than a wine-glass full, or nearly to a quarter of a pint of the liquor, I add a gallon of water; and mix it well together. I then sprinkle the whole of my trees over with this preparation with a brush, such as the plasterers use in moistening their walls, or sometimes by pouring it from a very small watering-pot with fine holes; beginning at the top of the tree, and laying it on very gently to prevent waste, which would be considerable if done with violence or thrown from an engine. Some

time

time after, either in one, two or three weeks, as I find it necessary, I repeat the sprinkling; and before the fruit gets to a size to be stained by it I go over them again, and have always found three times sufficient to secure them from the depredations of the insect, which generally preys on the leaves before the shoots are much advanced in strength. I have now practised this antidote for some years; and having during that time taken every opportunity of communicating the knowledge of it, I have at this time the satisfaction of seeing it in such general use around me, that I find our tobacco-manufacturer has such a demand for the liquor that he sells it at 1 *s.* 6 *d.* a gallon. It may, however, no doubt, be obtained at a very cheap rate in Bristol, and other places, where much tobacco is manufactured. I would farther add, that I have not confined my experiments of its use wholly to fruit-trees. Every vegetable and shrub which I have applied it to have been relieved by it, and restored to health, though ever so much injured by the insect tribe. I have no doubt but it would effectually destroy the red spider; and that it may be used with salutary advantage in numerous other instances. And as it is a remedy so cheap, and attainable in any quantity that may be desired, I hope it will prove, on being generally known, beneficial to the public. How far it may be practicable to use it in hop-grounds, or in other extensive views, I cannot say. But I should imagine, as one watering only has a most powerful efficacy, and as the labour of one man would in a day go far in the application of it, that considerable benefit may be derived from it wherever the insect preys.

I cannot dismiss my pen without mentioning a few words respecting another experiment, which my situation prevents my following up to the extent I could wish. I

shall therefore briefly state, that observing from time to time the immense produce of an ornamental grass, which I had much of in my garden, and which is distinguished in Miller's Dictionary by the name of Striped or Ribband Grass, it occurred to me, that Nature in her bounty did not bestow such a prolific quality on this beautiful grass, but for some wiser purpose than merely to gratify the eye. I therefore examined it attentively, and found it to be very succulent, and possessing much sweetness; and on offering it to my horses and cows, that they fed on it very eagerly. I had by me a calf just weaned, which I kept wholly by it for a month; and notwithstanding it had so recently been taken from its mother this grass supported it admirably, and I had the pleasure of seeing it thrive beyond my most sanguine expectations. I ascertained in the course of the year that I could cut it three or four times, and that its produce was always prodigious. It takes a very deep root, produces an early spring crop, and, I believe, is an excellent summer food for cattle; in the winter it disappears. I should imagine it may be raised from seed; but I have found it to be easily propagated by dividing the roots into smaller plants, and disposing of them at distances of from four to six inches. In moist ground it spreads rapidly, and soon forms a thick mass of food, exceeding any other kind I ever witnessed. Its durability is such, that what I have in my garden, which has been there to my knowledge these twenty years, is as thriving, and yields as much, as ever.

[*** It is presumed the destroying of insects by sprinkling with tobacco-water, is not new, though not generally practised; it may therefore be a public service to recommend the method. But as tobacco is comparatively a dear article, and the fluid

above

above mentioned not easily procured in many situations, it might be a public advantage of no small importance, if our ingenious correspondents would turn their attention to the different tribes of vegetables, with a view of finding among our most pungent and bitter plants, or by a cheap and easy mixture of them, a substitute for foreign tobacco, for such uses. The idea is not without promise.

And farther experiments on the striped grass are undoubtedly worthy of being made, in small inclosures near the farmer's or gentleman's house.—EDIT.]

Experiments on the Eudiometrical Means, and on the Proportion of the Constituent Principles of the Atmosphere.

By Messrs. HUMBOLDT *and* GAY-LUSSAC.

(Continued from Page 318.)

NOW as we have made a great number of experiments which prove, that when we employ them alternately in excess we constantly obtain the same proportions, it follows, that the combustion of hydrogene gas is of an uniform nature. The Galvanic phenomena of the decomposition of water seem however to prove that water is capable of oxygenating or hydrogenating itself; and it is upon this supposition that Messrs. Laplace and Berthollet have accounted for the singular experiment of the decomposition of water by two metallic wires immersed at one end in this liquid, and communicating by the other with the two poles of a Galvanic pile. But without wishing to controvert this explanation, which appears to us the most satisfactory that has hitherto been offered, we shall observe that the complete absorption of the hydrogene at one of the wires, and of the oxygene at the other, proves that the water does not become oxygenated or hydrogenated, because, in order to become

come so, it would be necessary that it should absorb one of the gases in a proportion greater than that required for the composition of water. If therefore it absorbs oxygene and hydrogene in exact proportions for forming water, it must be concluded that the properties of one of the gases are neutralized by those of the other. Accordingly, in the circumstances of which we are treating, the water might become instantaneously oxygenated at one of the wires, and hydrogenated at the other; but the two gases, being deprived of elasticity, and in exact proportions, must soon enter again into combination.

If it is well demonstrated that, under given circumstances, hydrogene or oxygene may be completely absorbed, and, if it is equally so, that the product of their combination is constant, nothing more remains to be done, in order to solve the third question, which we have proposed to ourselves, than to determine the proportions of oxygene and hydrogene which constitute water.

To 100 parts of oxygene gas we added 300 parts of hydrogene gas; and after having inflamed them by the electric spark, we obtained in twelve experiments the following residues:

100,8	101,0	102,0
101,4	101,7	102,0
100,5	102,0	101,0
101,0	101,5	101,5

The mean term of which is - - 101,3

Thus 100 parts of oxygene, supposed to be very pure, required 98,7 of hydrogene: but on placing our oxygene gas in contact with sulphuret, we found that it had all been absorbed within about 0,004; hence it follows, that 99,6 of oxygene absorbed 199,1 of hydrogene, or that 100 absorbed 199,89, or, to use round numbers, that

that 100 of oxygene requires 200 of hydrogene for their saturation.

In the preceding experiments it was the oxygene that disappeared. Let us now reverse the experiment, by inflaming a mixture of 200 parts of each gas; the residue of the different inflammations will be the following:

101,5	102,0	101,5
101,3	102,0	102,3
102,2	101,0	102,0
102,0	101,0	102,0
Mean term - -	101,7	
Mean absorption	298,3	

200 parts of hydrogene, supposed to be pure, required therefore 98,3 of oxygene; whereas, according to the proportion which we have just established, they ought to have had 100. But if we admit this same proportion to be exact in the 298,3 of absorption, there would be only 198,8 of hydrogene, which would indicate 0,006 of azote in this gas.

Supposing even the hydrogene to be perfectly pure, the two proportions obtained by causing the oxygene or the hydrogene to predominate, agree sufficiently with each other to confirm all that has been said in the course of this memoir: to render them identical, nothing more is necessary than to suppose 0,006 of azote in the hydrogene, in which, in fact, we can demonstrate its presence.

We have just seen, from the preceding experiments, that 200 parts of hydrogene, without making any correction, absorbed 98,3 of oxygene. Let us then take the residues 101,0 and 101,5, proceeding from the combustion of 100 of oxygene and of 300 of hydrogene, and detonate them with 200 of oxygene gas. In these two

residues

residues there must be 0,008 of azote belonging to the 200 parts of oxygene gas; and if the remainder 201,7 was pure hydrogene it ought to absorb 99,1 of oxygene, and consequently there ought to have disappeared by the inflammation 300,8 parts, but there disappeared only 295,0: it follows, therefore, that the residuum 201,7 was not pure hydrogene; and that, according to the proportion of 100 of oxygene to 200 of hydrogene, it contained 5,0 of azote, proceeding from 600 of hydrogene; that is to say, that this last-mentioned gas contained 0,008 of azote.

We therefore think it proved, that 100 parts, in volume, of oxygene gas require very nearly 200 parts of hydrogene gas for their saturation. According to the experiment of Messrs. Fourcroy, Vauquelin, and Seguin, 100 parts of the former would require 203 of the latter; but we shall observe, that, whichever of the two proportions we adopt, the error cannot amount to more than 0,0035 upon the absolute quantity of the oxygene of the air, and when the question respects relative quantities, the error will be still much smaller.

We have ascertained, that the proportion does not vary in consequence of changes of the temperature. It is evident that it must be so, since, as the heat equally dilates both gases, and causes them to dissolve equal quantities of water, the real weights of hydrogene and oxygene contained in equal volumes remain always in the same proportion. It would therefore be more accurate to say, supposing our proportion by the volumes to be well established, that 100 parts of oxygene require 200 of hydrogene, than to indicate the proportions of water by the weight. If the oxygene and hydrogene employed for the composition of the water had been perfectly dry, or if a correction had been made according to the quantity of moisture

moisture which they might contain, it would be a matter of indifference whether the proportions of its principles were enunciated according to their volumes or their weights; but since hydrogene combines with oxygene in double the volume of the latter, and as they both dissolve the same proportion of water, it is evident that they do not carry into the combination quantities of water which are in the same reciprocal proportions as the quantities, by weight, of oxygene and of hydrogene, and that consequently the proportion of the principles of the water must thereby be altered. Thus the proportion, according to the volumes, has the property of remaining constant, notwithstanding the changes with regard to temperature and moisture, whilst that, according to the weights, varies under the same circumstances. And let it not be imagined that this consideration is of little importance: for it would be very easy to prove that it has very considerable influence upon the proportion of the principles of water. According to the experiment of Messrs. Fourcroy, Vauquelin, and Seguin, the most accurate that has hitherto been made upon this subject, water contains by weight 85,662 of oxygene and 14,338 of hydrogene. But the experiment having been made at a temperature of about 14°, and the correction due to the water held in solution by the gases not having been made, it follows, that if we adopt their specific weight of the oxygene and hydrogene gases, as well as the proportion of their volumes in their combination, and moreover admit with Saussure, that a cubic foot of air, at the temperature of 14°, contains very nearly 10 grains of water in solution, the proportion by weight between the oxygene and hydrogene, instead of being 85,662 to 14,338, would be 87,41 to 12,59; a very remarkable difference, and which especially must be of great in-

fluence in analyses where the real weight of the hydrogene is to be determined. The same consideration applies also to the specific weight of the gases, and principally to that of the hydrogene, of which about a sixth part is due to the water which it holds in solution when the temperature, as here supposed, is 14° Reaumur. We do not therefore doubt, that if we had hydrogene gas perfectly dry, and deprived of the azote gas, which appears very often to accompany it, we should find its specific lightness to be at least 15 times greater than that of the atmospheric air.

It still remains for us to answer the last question which we have proposed to ourselves, and to shew what are the limits of the error in Volta's eudiometer; and next, what are the smallest quantities of oxygene or of hydrogene that can be estimated by his method.

The effects obtained with this instrument, being instantaneous, are independent of the thermometer and barometer. In this point of view, it has the very decided advantage over phosphorus and the alkaline sulphurets, of giving results very capable of comparison; but this is not the only one; it has also that of the eudiometrical means which give multiples of the quantity to be estimated. As in this instrument each hundredth of oxygene is represented by a three times greater absorption, the error that can be committed amounts only to a third upon this gas; and now, especially as we have instruments which divide the measure into three hundred parts, it is evident, that if we err even by one division, the accuracy, as to the quantity of oxygene, may be carried to near a thousandth part of the quantity of air analyzed.

If therefore the results of the combustion of hydrogene gas admit so well of comparison, and the errors to which they

they are liable are restricted within such narrow bounds, it is evident that we may not only find the slight differences which exist between two portions of atmospheric air, but also determine less than three thousandth parts of oxygene which shall be lost in azote or hydrogene gas: but in this case, in order that the inflammation take place, it would be necessary to add a given quantity of oxygene, the absorption of which with hydrogene gas had been determined by previous experiments, and then the excess of the first absorption above the second would be attributed for a third to the oxygene gas contained in the air which is analyzed.

On the other hand, in order to determine whether one portion of hydrogene be more pure than another portion, or whether there exist very small quantities of it in a gas or in the atmospheric air, it would be necessary, in the first case, to mix 100 parts of hydrogene gas with 100 of oxygene: the quantities of real hydrogene would be in the direct ratio of the absorptions. But if the proportion of hydrogene were very small, for example one two hundredth, it would be necessary, in order to effect its combustion, to add 100 parts of this gas to 200 of the air to be analyzed, and to detonate the mixture with a sufficient proportion of oxygene. By this means, and with the practice which we have now acquired, we have been able to find again three thousandth parts of hydrogene gas which we had mixed with atmospheric air.

It might be objected against Volta's eudiometer, that the hydrogene not being always the same, we might incur errors difficult to be appreciated. We shall first observe that it is indifferent whether or not it contain azote; but if it contained oxygene, its quantity confounding itself with that which we wish to appreciate, would alter the results. In order to avoid this inconvenience, we may

first of all detonate separately 500 parts of hydrogene with 100 of oxygene; by this means its oxygene will be destroyed, and we may then employ it for the analysis of the air. With this precaution we may employ a gas prepared as inaccurately as possible. It is sufficient if it have been extracted from the water by means of zinc and sulphuric acid or muriatic acid; for it is well known that if we employ another metal, such as iron, the gas is no longer of the same nature.

After all the experiments which we have recorded, we might have been justified in concluding that the eudiometer of Volta must indicate the whole of the oxygene contained in the atmospheric air; but we wished to determine this point by direct experiment. We analyzed an air composed of 20 parts of very pure oxygene and 80 of azote, obtained from the decomposition of ammoniac by oxygenated muriatic acid, all possible precautions being taken to prevent its becoming mixed with atmospheric air. 200 parts of this air, inflamed with 200 of hydrogene gas, gave five absorptions, the greatest of which differed from the smallest only by 5,1000 and the mean term of which was 124,9. These 124,9 parts indicate 41,6 of oxygene, of which the half 20,8 corresponds to 100 of our factitious air. We find therefore 0,008 of oxygene more than we had employed, which might seem to indicate that the proportion of 100 of oxygene to 200 of hydrogene is rather too great; but we must observe that our hydrogene, though carefully prepared, still emitted light with phosphorus, and that in order to explain our result, it is sufficient to suppose that the azote contained a hundredth part of oxygene, which is sufficiently probable if we consider that the oxygenated muriatic acid is very speedily decomposed by the action of light.

It appears from what has been said, that the results afforded by Volta's eudiometer admit very well of comparison,

rison, and that the limits of their differences may be reduced very nearly to a thousandth part of the air analyzed. It appears also that by its means we may estimate very minute differences between two airs or very small quantities of hydrogene mixed with the atmospheric air. Independently of the property which this instrument possesses of indicating the whole quantity of oxygene contained in an air, it is the only one with which we can ascertain the proportion of hydrogene in a gaseous mixture, and in this point of view it still merited to have its mode of acting attended to and studied.

Thus the illustrious Volta, to whom Natural Philosophy is indebted for so many beautiful discoveries, has also the merit of having furnished Chemistry with the most accurate and valuable instrument for analyses.

Analysis of the Atmospheric Air by Volta's Eudiometer.

As we have now proved that Volta's eudiometer gives results very capable of comparison, that it can indicate the whole quantity of oxygene contained in the air, and that it has over the other eudiometrical means in which the absorbing substance is solid or liquid, the advantage of giving multiples of the quantity of oxygene to be estimated; we shall proceed to apply it to the analysis of the air. If the proportion of 100 of oxygene and 200 of hydrogene which we have established be strictly correct, we shall obtain the proportion of the oxygene to the azote likewise correct; but supposing even that the quantity of hydrogene were too large or too small by 5 parts, the error would not amount to more than three thousandth parts of the air analyzed, and we should have the advantage of obtaining a greater precision than by any of the other known eudiometrical means.

The

The atmospheric air which we have analyzed was collected over the middle of the Seine in cold, temperate and rainy weather, and during the prevalence of different winds. In order to obtain a greater parity between the circumstances, and better to appreciate the differences in the nature of the air, if there were any, we analyzed on the same day the different portions of air which we had collected in different weathers, and which we had preserved in glass vessels well closed and inverted over the water. For brevity's sake, we have comprized in the table annexed to this memoir, the absorptions produced by the inflammation of 200 parts of air and 200 of hydrogene gas, and have at the same time indicated the correspondent quantities of oxygene.

It will there appear that our experiments prove, in the first place, that there are no variations exceeding a thousandth part in the quantity of the oxygene of the air, though that which we analyzed, having been collected during the prevalence of different winds, came from regions very remote from each other; and in the second place, that the proportion of the volume of the oxygene to the other gases which exist in the air, is as 21 to 79. The first result, that the air does not vary in its composition, is rigorously exact, because it is independent of the proportion of the hydrogene and oxygene gas which constitutes water; but the second result, that the air contains 21 hundredths of oxygene, can also deviate but very little from the real truth; for if we suppose that the quantity of hydrogene required for saturating 100 parts of oxygene were larger or smaller by 5 parts than that which we have assigned (and which we have reason to believe is correct within a much less difference), the error with respect to the proportion of oxygene which we have found in the air, would not amount, as we have already observed,

served, to more than three thousandth parts of the air analyzed.

But as many meteoric phenomena may be attributed to inflammation of hydrogene gas, it has been endeavoured to explain them by admitting the existence of this gas in the atmosphere. We therefore thought it a very interesting enquiry, to investigate whether the air actually contains hydrogene gas; and in order the more easily to discover it, we made a gaseous mixture, in which we were sure that there was none of it, and we made a comparative analysis of the two airs. We made a mixture of 20 parts of oxygene and 80 parts of azote, obtained from ammoniac by means of the oxygenated muriatic acid, and we detonated 300 parts of each of the two airs with 100 of hydrogne; but the result of six experiments made with the atmospheric air was exactly the same as that of six others made with the factitious air. And as we have shewn that we could appreciate less than three thousandth parts of hydrogene, it must be concluded that the atmosphere does not contain this gas, or if it contains it, its quantity does not amount to three thousandth parts. It cannot however be doubted that there exists a little hydrogene in the air; some is daily evolved from marshes, but its quantity may be so small (a thousandth for example) as to elude all our means. The proportion of carbonic acid existing in it ought to be much more considerable, if we reflect upon the abundance of the sources which furnish it, and yet, if it did not form insoluble combinations with lime and barytes, it would perhaps still remain to be ascertained by the determination of its volume whether any of it existed in the air. The carbonic acid, it is true, cannot accumulate in the air, because vegetation decomposes it; but is it proved that there do not exist causes which return the hydrogene

to

to the earth, and thereby prevent it from accumulating in the air?

To draw a conclusion from the preceding experiments, we shall say: 1, that the atmosphere does not vary generally in its composition; 2, that the quantity of oxygene which it contains is 21 hundredths; 3, finally, that it does not contain any hydrogene which we are able to appreciate.

This identity of composition in which the principles of the atmosphere are constantly maintained, and this absence of hydrogene which our experiments prove, must give confidence to the Geometrician with respect to the theory of refractions. The refractive power of the different gases being different, and that of hydrogene being stronger than that of oxygene and of azote, the theory of refractions, which is founded only upon the variations of the barometer and thermometer, would be very imperfect if the atmosphere changed in its constituent principles; but fortunately it is easy to prove that these changes do not take place in a sensible degree, and that the hydrogene gas, the refractive power of which is very great, does not exceed 0,003, at least as far as the greatest heights to which men have ever ascended. The Geometrician will therefore have nothing else to consider in the Theory of Refractions than the barometer, the thermometer, and the hygrometer.

In fact, a little reflexion will be sufficient to convince us that the atmosphere cannot vary very considerably in the space of some years, and still less of some days, at least if we do not speak of some very particular local variations. For if it varied thus in so short a space of time, by what miracle should it do so, and return suddenly to its original state? How conceive a cause sufficiently powerful to change from one day to another the proportion of oxygene by a thousandth part only, unless we should admit the existence of a magnetic or electric power, or any other

ther equally imaginary, which could change, by unknown modifications, the oxygene into azote, and *vice versâ*? It is possible that the atmosphere varies very slowly both in the proportion of its principles and in its weight; but these variations, for being so insensible, ought not the less to engage the attention of philosophers.

If it is now well proved that in general the atmosphere does not vary in its composition, we must seek for the cause of the differences which some have imagined they had discovered in it, in the local circumstances under which it was analyzed. Volcanoes upon high mountains, particular fermentations, water issuing from a marsh or a lake, might perhaps in some degree impair the purity of the air in contact with them, either by depriving it of oxygene, or by exhaling into it non-respirable elastic fluids; but how trifling must not this diminution of the proportion of the oxygene be in so large a mass of continually agitated air, when we consider that in places where a great number of individuals is collected, or in those where there seems to exist a focus of infection, the air nevertheless experiences but very slight variations. We have analyzed two portions of air, one of which was collected in the pit of the Theatre Français, immediately before the curtain was raised for performing the after-piece, three hours and a half after a great number of spectators had assembled, and the other of which was collected three minutes after the entertainment had ended in the most elevated part of the house. These two portions scarcely rendered lime water turbid; the atmospheric air indicating 0,210 of oxygene, the air of the pit indicated only 0,202 and that of the highest part of the house 0,204.

Analysis of the atmospheric air.	Of the air of the pit.	Of the air of the highest part of the building.
200 atmosph. air. 200 hydrogene.	200 atmosph. air. 200 hydr.	200 atmosph. air. 200 hydr.
126 air absorbed. 21 oxygene.	121,5 abs. 20,2 oxyg.	122,5 abs. 20,4 oxyg.

M. Seguin has also analyzed the air of hospital wards which he had kept closely shut up for the space of twelve hours, and found it to be almost as pure as the atmospheric air, although it had an insupportably infectious smell.

If therefore, even under circumstances the most favourable to the absorption of the oxygene, the air does not lose one hundredth part of it, we cannot thereby account for the sense of anxiety which we feel in close and crowded apartments, or the maladies which are peculiar to the vicinity of lakes and marshes, or to certain countries. Under some circumstances they will be produced by emanations which elude all our eudiometrical means, and which act in a peculiar manner upon the human body. Thus a bubble of sulphurated hydrogene gas, of oxygenated muriatic acid, a putrid exhalation, even a flower, may fill an immense space with this odour, and astonish our imagination by their extreme subtility, even when we are ready to sink under their action. The pestilential miasmata may be equally subtile without being the less deleterious, and equally elude all our means of analysis. Fortunately, if we cannot seize these atomic substances and determine their nature, we may at least, after the labours of M. Guyton, which have been productive of such great benefit to mankind, destroy their action. But under other circumstances the maladies may arise from the

the humidity of the air, from its temperature, from its electric state, or in general from the state of the atmosphere with respect to the peculiar state of the individual affected; and in these cases, which may be very frequent, the malady may make great ravages without its being possible to arrest its progress: it would therefore be illusory to attribute all to a single cause, when the state of human health depends upon the concurrence of all the circumstances under which men are placed.

But let us now recapitulate the principal facts contained in this first part of our Memoir, and call to mind some of the explanations which we have offered, hoping we may be permitted to consider them as expressing the real state of the facts.

The solution of an alkaline sulphuret, when made cold, does not absorb the azote, and it may be employed with advantage for the analysis of the air: when made hot, it absorbs it, and indicates a greater diminution of volume in the air than that which proceeds from the absorption of the oxygene. It is to the water only, and not to the sulphuret, that we are to attribute this property.

There are certain proportions of oxygene and hydrogene in which the combustion produced by the electric spark may be complete; there are others in which the combustion ceases before being completed; and finally there are others in which it cannot take place at all. These last phenomena seem to depend upon the circumstance that the temperature necessary for the combustion is not sufficiently elevated, and not upon the mutual affinity of the gases: for in all cases in which the combussion is not complete, we need only raise the temperature artificially in order that it may become so. When the hydrogene and oxygene are not entirely absorbed, we

find them again in the residues, and prove that they have not formed new combinations.

When we cannot inflame a gaseous mixture which contains oxygene and hydrogene, it will be sufficient to augment the proportion of these two gases. The meteoric phenomena cannot be results of the inflammation of hydrogene gas, since even in an air consisting merely of pure oxygene, it would require more than 6 hundredths of hydrogene for combustion to take place, and even then it would be only partial. Electricity seems to act in the inflammation of oxygene and hydrogene gas by the heat arising from the compression which it produces in its passage through their mixture. These two gases, by their combination, form water, which is constant in its nature. If the galvanic phenomena seem to prove that water is susceptible of oxygenation or hydrogenation, they may be equally well accounted for without the aid of such an hypothesis.

One hundred parts by volume of oxygene require for their saturation 200 of hydrogene. This proportion is independent of the changes of temperature and moisture, whereas that determined by weight varies under the same circumstances, because the two gases do not carry into the combination quantities of water which are in the same proportion as their quantities by weight; whence it results that the proportions of the water which have been established must be modified. The Eudiometer of Volta is capable of indicating the whole quantity of oxygene contained in a given volume of air within about a thousandth part of that volume, and its results admit very well of comparison. In the present state of our knowledge, it is the most exact of our eudiometrical means; it is not only capable of indicating very small quantities of oxygene or of hydrogene, and determining the purity of the last-mentioned

mentioned gas, but it has likewise the advantage of giving multiples of the quantity to be estimated. It has therefore in all these respects a very decided superiority over the other eudiometrical means. The atmosphere contains only 0,21 in volume of oxygene, and it does not vary in its composition. It contains no hydrogene, or if it contains any, its quantity cannot amount to 3,003.

TO BE CONCLUDED IN OUR NEXT.

Account of a new Varnish. By M. PARMENTIER.

From the ANNALES DE CHIMIE.

THE Apothecary-General of the French Military Hospital at Genoa, M. Bompoix, having sent me some coffee-cups, the merit of which seemed to consist in their lightness, and especially in the varnish with which they were covered, a varnish which was held in high estimation, possibly on account of its composition being kept a secret in the country, I solicited M. Bompoix to endeavour to discover the receipt, which he at length obtained from one of his pupils, who availed himself of his acquaintance with the manufacturer to procure it. It is this:

Take linseed-oil	1½	lbs.
Yellow amber	1	
Litharge in powder	0	5 oz.
Minium ditto	0	5
Ceruse ditto	0	5

Boil the linseed-oil in an unglazed pot; have a small linen bag, of a size sufficient to contain easily the litharge, minium, and ceruse, which tie in it, and suspend in the pot, so as not to touch the bottom. Continue the

the ebullition till it begins to turn brown; then take it out, and continue to boil the oil, adding a clove of garlic, peeled, and when it is dried add another, and so on to the number of six or seven: have then, in an unglazed earthen pot, the amber fused in the manner about to be described, and when it is fused pour the whole into the linseed-oil.

To fuse the Amber. Take about two ounces of linseed-oil, to moisten the amber and facilitate its fusion by a very violent fire; and when it is fused mix it with the linseed-oil, and boil it for about two minutes; then take it out, and strain it through a piece of linen, of a rather open texture; after it has cooled put it into a bottle, and cork it well, to prevent its drying.

Manner of using it. Take the article you wish to varnish and polish it well; then apply the varnish in the following manner. Take lamp-black, varnish, and a small quantity of essence of turpentine, and mix them together with a brush. Apply a coat of this mixture to the article that is to be varnished; and when this coat is dry apply another, and so on to the number of four: when they are dry place the article in a stove, or at the fire, in order to dry it completely, and then polish it with pumice-stone and whiting.

Manner of preparing the Article to be varnished. Take walnut, alder, or cherry-tree wood, because these woods are porous, and when perfectly dry do not warp any more: first shape the piece, and put it to dry in the stove; then work and polish it, as if it were finished; finally, varnish it as above directed.

If you wish to give a red tinge to the ground of the cup, throw into the varnish a small quantity of minium, or better of cinnabar; and proceed in like manner for any other colour you wish to give to the article.

On

On the Manner of obtaining distilled Spirit from Potatoes.

Translated from the GERMAN in the ANNALES DE CHIMIE.

IT has long been customary in Germany to obtain distilled spirit from potatoes, with the aid of proper preparations and causing them to ferment. In the Eastern part of Prussia, and in Lithuania particularly, a great quantity of potatoes are applied to this use. As, in those countries, they are commonly cultivated in land recently cleared, they yield abundantly; especially if they have been cultivated in a proper manner, and the weather has been favourable. The lees of the potatoes, or the residue of the distillation, affords an excellent drink for cattle, especially for cows, which it causes to give more milk. Potatoes, as every one knows, are, also, an excellent aliment for the human species; on which account the cultivation of this root is extremely advantageous, and greatly diminishes the consumption of grain, formerly employed for producing the same quantity of distilled spirit, which now is obtained from potatoes. When this spirit is well distilled, and not mixed with any foreign substances, and when the potatoes have not been burnt or too much heated in their preparation, it has a very palatable taste; much superior to, and different from, the bad spirit obtained from barley or wheat; which, however, some persons prefer, on account of their being habituated to it.

It has been pretended that the spirit of potatoes easily turns sour, and that it cannot pass the line without spoiling: as I cannot prove the contrary, I have nothing to say to these assertions. I know, however, that this spirit will keep at least eighteen months, and, that after being kept

kept for that space of time, it indicated, by Richter's alcoholometer *, the 35th degree, without having lost any thing of its quality, or turned sour. I have, therefore, reason to believe, that this spirit, when well prepared, is not more liable to the two disadvantages which have been laid to its charge, than that of grain, and that whatever has been objected against it is the effect of prejudice.

Manner of obtaining distilled Spirit from Potatoes.

This spirit is obtained by adding to the potatoes a sufficient quantity of malted barley; for example, to a hundred Berlin bushels of potatoes seventeen bushels and a half of malted barley are added, which produce five hogsheads of spirit, the hogshead containing two hundred and forty German quarts, which, by Richter's areometer, marks from 36° to 38°.

I know also, by experience, that with one hundred and twenty bushels of potatoes and ten of malt, we may obtain the same quantity of spirit, of equal strength. It might, perhaps, be imagined, that in order to produce the same quantity of spirit from potatoes, it would require a greater quantity of ingredients proper for fermentation than are required for the spirit of grain, as also more room, more casks, and a greater expence in fuel; but this is not the case, for the same casks which contain a given quantity of corn, contain, very nearly, the same quantity of potatoes and malt, which produces also the same quantity of spirit, provided that care has been taken

* Richter's alcoholometer is the same instrument as Baumé's, with only this difference, that each degree has been fixed according to experiments made expressly for the purpose. The scale is graduated from 1 to 100, in such a manner, that the number, at the level of the liquid, indicates the quantity of alcohol:—thus 36, indicates 36 parts of alcohol in 100 of the liquor.

at

at first to make the potatoe mash thicker, because they do not, by any means, possess the same property of thickening that corn does, and that, also, the fermentation has taken place as it ought, and the spirit has not been burnt in the alembic. As to the additional quantity of wood required for boiling the potatoes, this expense is scarce worthy of consideration, since, in all cases, boiling water is required. This water is employed for boiling the potatoes, which is effected by means of the vapour of the water, which must be kept in ebullition for half or three quarters of an hour longer, according to the quantity of potatoes employed; and this is all the additional expense in fuel, that is required.

The boiling of the potatoes is performed in a cask of oak wood, the staves of which must be very strong and thick, and the heads hasped with iron, in order to prevent their being injured in stirring; the upper head has a square aperture with a very strong lid which closes completely: this aperture serves to introduce the potatoes into the cask after they have been washed and cleaned: there is also another aperture, but smaller, which has likewise a door that closes well, and is intended for taking the potatoes out of the cask.

This cask is placed upon a trestle at the side of an alembic, different from that used in great and small distillations. At the same side, that is to say, opposite to the alembic, over the bottom of the cask, there is an orifice for placing the neck of the head of the alembic, and through which the distillation is to pass. In the middle of the lower end there is, also, a small orifice for evacuating the thick fluids which have collected in the cask, and in order that the potatoes may not stop it up, it must be covered in the inside of the cask. When the potatoes are boiled, which the workmen may ascertain by means of the door in the side of the cask, the neck of

the alembic is withdrawn from the cask, and the grinding of the potatoes is immediately commenced with a machine, or a kind of a hand mill, placed before the cask, close to the small door at the side. This mill is composed of two cylinders of very hard wood, or of sandstone, the axles of which are turned towards each other, by means of a wheel worked with a handle. Over the cylinders there is a coffer, or hopper, into which the potatoes are put as they are successively taken out of the cask with a ladle, which being ground by the action of the cylinders, fall into a vat placed below them. What renders this vat indispensably necessary is, that below each cylinder there is an iron scraper for removing the potatoe mash which attaches itself to the cylinders.

When the potatoes are boiled, ground barley is put into a vat and diluted with warm water, taking care to make the mixture rather thin; the potatoes are then added, by tub-fulls, as fast as they are ground, and when they are entirely done, the mixture is well stirred and the requisite quantity of water added, till the mash is perfectly smooth and no more lumps remain in it. It is then left to stand, stirring it however, from time to time, till the whole is cold, and the yeast can be added to it.

In some places only yeast is employed; but, in others, they use also an artificial ferment, prepared simply of rye, ground and well cleaned. This last ferment is made by first forming a paste with the ground rye and cold water, to which boiling water is, afterwards, added, in such quantity as to form a thick *bouillie;* this is mixed first with yeast, and, afterwards, with artificial ferment, at several times, whereby a fermentation is excited in the mass. The whole art consists in the manner of heating this preparation, for the greatest care must be taken not to spoil the mass by heating it too much.

It

It is also to be observed, that potatoes, as they ferment more readily than corn, require a less quantity of yeast; moreover, the fermentation is very violent, producing a copious and continued formation of scum; but it does not take place every where in a similar manner; for, in certain places, the gross and membranaceous parts of the potatoes form a strong crust at the top, through which the scum can penetrate but seldom, or not at all, without which, however, the fermentation is stopped, or interrupted. Experience proves that by distilling potatoes with beet roots and carrots, a better and more abundant spirit is obtained, than when only potatoes and beet are distilled together; but the advantages which some have attributed to the latter method of proceeding have not been confirmed; on the contrary, it has been proved that when carrots have been added to the distillation, the spirit obtained has a very agreeable smell and taste.

That kind of cabbage which the Germans call *Kohlrabi*, the French *Choucave*, is a species of *Brassica napus*, and which, growing in the earth, differs from that which grows above the ground in a round form, produces a bad spirit, of a very disagreeable smell and flavour. Its fermentation is neither so strong, brisk, nor continued: the preparation quickly turns sour, on which account the spirit soon acquires a disagreeable taste, and becomes covered with a whitish mould. The lees of this brassica, or the residue of the distillation, must not be kept for any time before it is given to the earth, for it spoils very soon, even in Winter: neither do they relish it so much as that of the potatoes and beet root.

Chemists pretend, that it is the saccharine substance, or juice, that gives rise to the vinous fermentation; that the more a substance contains of this saccharine substance the better adapted it is for fermentation; but the results obtained with potatoes seem to prove that their assertion is

by no means grounded in its full extent, for potatoes contain no saccharine substance, but merely starch, and yet they ferment. It is evident that in grain the quantity of spirit is in proportion to that of the starch, or, perhaps, of that glutinous substance which it contains; wheat, for example, which contains the most of both, furnishes also the largest quantity of spirit.

The opinion of those who urge that wheat acquires by germinating a sweet taste, does not seem to me to be of any great weight or signification; for our predecessors had no very clear or accurate ideas on this subject; besides which the point has not yet been fully ascertained, by rigorous experiment and comparison, of the different species of grain, when they have, and when they have not germinated; for there are several well-informed distillers who still doubt whether a given quantity of any species of grain produces a larger quantity of spirit, of equal strength, on account of the grain having germinated.

On Vapour Bleaching. By M. CURADEAU.

From the BIBLIOTHEQUE PHYSICO-ECONOMIQUE.

THE experiments on vapour-bleaching, which I lately made at the Hospital of Incurables, having proved completely successful, I think it incumbent upon me to give such details of the process which I employed as may enable the public to appreciate the advantages which this new method possesses over the former one.

The trial was made upon eighteen hundred pounds of dry and very foul linen, consisting of sheets, table-cloths, napkins, shirts, and dish-cloths; it had merely been steeped the evening before in the water of the hospital cistern, without any soap being employed in this operation, a point upon which I particularly insisted.

This

This linen, after having been thus steeped and wrung out, was impregnated with ley prepared with American potash, but without the addition of soap; for experience has shewn me, that this substance is not only superfluous, but that it also has the disadvantage of modifying the effects of the alkali, which in many cases has prevented the success of the operation.

The quantity of potash employed for the leying of these eighteen hundred pounds of linen was forty-five pounds, which gives two pounds and a half to the hundred of linen. However, as each kind of linen was more or less impregnated with grease and filth, it became necessary to make a selection, in order that a ley might be separately applied to each kind of linen, of a strength proportioned to its state. In this manner therefore I was certain of applying the alkali to its proper use, which is much more advantageous than the old method, which subjects the muslin handkerchief and the dish-cloth to the action of the same ley. Here this inconvenience is avoided, since every kind of linen has its particular ley, and by its peculiar arrangement in the vapour-vat, the ley of the dish-cloths cannot communicate with that of the fine linen.

The quantity of water employed for the preparation of the ley might be equal to about two-thirds of the weight of the dry linen.

About eleven o'clock in the forenoon the linen was begun to be put into the vat; the dish-cloths were placed at the bottom of the vessel, then the table-cloths and napkins, and, lastly, the shirts and sheets. It was now about half-past twelve o'clock; when the fire was immediately kindled, and continued till it was ascertained that the heat communicated to the linen was uniformly diffused, and equalled 80 degrees. At nine o'clock it was perceived that the operation had been continued for a sufficient

sufficient length of time, and the application of the fire was consequently suspended. For this leying only 214 pounds of wood had been burnt; the value of which was estimated at 4 francs 50 centimes. Part of this expense might have been saved, if care had been taken to remove the charcoal. Nevertheless, at nine o'clock the following morning, a quantity was taken out, amounting in value to 1 franc 35 centimes.

On the morning of the second day the linen was taken out of the vat, in order to be washed. This was done in the presence of many persons of both sexes, who all agreed that they had never known a ley wash better, or render the linen whiter. An additional circumstance in favour of this method was the facility with which the spots were removed; moreover, for so considerable a quantity of linen only a pound and a half of soap was employed, which in common circumstances might very well not have been employed at all.

This method appears, therefore, to possess the following advantages: 1, it is continued; 2, linen bleached in this manner is much whiter, and more easily washed, than according to the former method; 3, the vermin which is so often found among the linen of hospitals, and which is not destroyed by the action of ordinary leying, as the directors of the hospital have remarked, will be completely destroyed by the heat of 80 degrees, which our method is certain of giving to the linen; 4, finally, a saving of two-thirds of the expense is obtained upon the articles of fuel and soap. As to the saving of potash, I do not propose any; for whether this substance acts in much or in little water, the case will always be the same with respect to the substances that are to neutralize its action.

I must here remark, that sheets which were leyed for the first time in this experiment were much whiter than similar

similar ones which were compared with them, and which had already undergone the action of three ordinary leys. This observation, to which I paid particular attention, proves that unbleached linen will sooner become white by this process than that which has been subjected to the action of ordinary leying.

According to the information which I have collected in establishments where this operation is conducted with the greatest economy, the same quantity of linen would have required 45 pounds of potash, 5 of soap, and 600 of wood. In how many other establishments will not even this expense be exceeded?

In our experiment the quantity of these substances employed was, potash 45 lbs. wood 214, soap 1½. As to the saving of manual labour, with which it is attended, we cannot as yet determine it, but must leave it to be ascertained by farther experiment.

Considerations on the Oxydation of Metals in general, and on that of Iron in particular. By M. THENARD.

From the ANNALES DE CHIMIE.

As soon as oxygene was discovered, philosophers occupied themselves with the investigation of its properties, and it was not long before they found that this gas is the general agent of combustion. The phlogistic theory then disappeared; and it was no longer necessary, in order to explain most phenomena, to build hypotheses in every respect contradictory to experience. By admitting a greater or less proportion of this new principle in the metallic calces, an exact account was given of the augmentation of weight which the metals acquire when

when they are calcined. But this theory, which now appears to us so simple, is nevertheless the fruit of great efforts of genius; and yet when we compare it with the ancient theory, we find it difficult to decide which of the two confers most honour upon its author. On the one hand, Stahl brought forth science as it were out of chaos; all the elements of which it was to be formed were in confusion; he separated, he attentively investigated their properties, and at length he established among the phenomena which they present, such a connection as rendered them all dependant upon each other. On the other, Lavoisier, by a surer and more scientific progress, goes to the bottom where Stahl only skimmed the surface; one might say that he sees the constituent particles of matter re-act; he learns the art of following them in their union and in their separation; he gives nothing to hypothesis, he consults only experience; in place of an imaginary he substitutes a real substance; he discovers the general laws of nature, and traces the route that must be followed by his contemporaries and successors. Thus the one opened the path which the other travelled over; but if Stahl had not existed, Chemistry would perhaps be still in its infancy; at least it must be admitted that his doctrine has had the greatest influence upon the creation of the new theory, even in the very circumstance that it was entirely opposite to his principles; and in fact would he not have designated oxygene gas, if he had made his phlogiston to perform the reverse of the part which he assigned to it? He therefore presented, by the force of his genius, the existence of an analogous matter; a matter which, it is true, he never was able to obtain, but which he nevertheless admitted, and to which he attributed imaginary properties, in order especially to explain the phenomena of combustion.

When

When it was well demonstrated that in combustion the metals as well as the other combustible bodies, far from losing one of their principles, absorbed a new one since they increased in weight; when, in short, Lavoisier had taught us that in this phenomenon, the cause of which was so long unknown, the air was decomposed, and that one of its elements entered into a new combination, the properties of burnt bodies were investigated with greater attention; a great number of new ones were discovered, and with respect to several of them at least, the quantities of oxygene and of radical which constituted them, were ascertained. These new considerations were again the source of many discoveries. It was seen that the same combustible body might combine in different quantities with oxygene; and that consequently several oxyds, as well as several acids, might have the same radical. We shall meet, especially in the oxydation of metals, with frequent applications of this principle; and of late lead, antimony, and manganese have presented us with some very remarkable ones. It was this variety of oxyds which leads the author of Chemical Statics to think that there was not so great a difference as had till then been conceived between the oxyds of the same genus, and which suggested to him the belief, supported also by reflections which are the result of a great number of experiments, that probably even the metals pass from the metallic state to the maximum of oxygenation by going through all the intermediate degrees of oxydation; so that there would be for each metal a multitude of different oxyds.

I am well persuaded that the number of the metallic oxyds is much greater than what is admitted by the majority of chemists, and that they do not pass immediately,

as is supposed, at least with respect to some, from a low degree of oxygenation to a very high one; that between these there exists one or more intermediate degrees which constitute as many particular oxyds; but I confess I am not yet convinced that there are as many oxyds as possible degrees of oxygenation; and if theory admits, experience seems to reject them; indeed, why should these different oxyds not combine with the acids? The cause might perhaps be attributed to this, that the metals, at a certain degree of oxydation, have more affinity for the acids than at any other; but for this very reason, it is possible that, combined with a given quantity of oxygene, they form permanent oxyds, whilst, with a greater or a smaller, these oxyds can have only a momentary existence. The latter will therefore be placed between the former; they will be as many degrees through which the first will be obliged to pass without being able to remain stationary, and this is precisely what we observe with respect to the acids that have the same radical. Will it be said that between the sulphurous and the sulphuric acids, between the phosphorous and the phosphoric acids, there are many other intermediate acids? And if such intermediate acids do not exist, if sulphur, phosphorus &c. can form only two acids, why should the metals give origin to a multitude of oxyds; why should there not be for the one as well as for the other fixed degrees only of oxygenation? How should it come to pass, finally, that hydrogene forms only an oxyd, although it is capable of fixing more than five and a half times its weight of oxygene? There are so many reasons which induce us to believe that the metals are entirely in the same predicament with the other combustible bodies; and since it is proved that these cannot form with the general

principle

principle of combustion a multitude of different combinations, I am persuaded that the metals, which are justly regarded as analogous bodies, are not more susceptible than they of a multitude of different oxydations.

But if it may be permitted to advance some doubts against this manner of thinking, none can be entertained respecting the nature of the oxyds in their combinations with the acids; and if this truth had not been long admitted by chemists, some general observations would suffice to place it in its full evidence. Let us, nevertheless, cast a glance upon each of these oxyds, and attentively consider the oxyds of iron, which it is more particularly my purpose to examine in this memoir. Here in fact every thing leads to prove that in combinations of this sort, the oxyds are constant. Though cobalt, nickel, bismuth, lead, zinc, gold and platina, are all the base of several oxyds, nevertheless, in all the salts which they form, they are always equally oxydated: thus the oxyd is blue in all the salts of cobalt, green in those of nickel, white in those of bismuth, of zinc and of lead; it is grey in those of silver, yellow in those of gold, and brown in those of platina; it varies indeed in those of antimony, of tin, of mercury, of copper and of iron; but still with several of them it is only a few of their oxyds that can unite with the acids. Only two white oxyds of antimony are susceptible of this combination; the volatile white oxyd and the white oxyd of the second degree; the white oxyd at the maximum is attackable only by the muriatic acid; moreover unless it be in a highly divided state, the muriatic acid dissolves it with very great difficulty, and always by passing in part into the state of oxygenated muriatic acid. Tin, mercury and copper, like antimony, form saline combinations with the acids, only in two states

of oxydation; mercury in the state of black oxyd and of red oxyd, and copper in the state of yellowish white oxyd and brown oxyds.

Hitherto it has been thought, that the case was the same with iron; that in all the salts which it was capable of forming, the oxyd was always either green or red. Some Chemists, however, have admitted an intermediate oxyd; they have believed that there was also among them a yellow oxyd, because, the ferruginous salts sometimes present themselves of this colour. Reasoning according to the hypothesis, that all the metallic salts are always of the same colour as their oxyds, such a conclusion was perfectly consistent with the principles adopted; but it is very well known at present, that this mode of considering the matter frequently leads into error, and that one salt is white, or rose-coloured, though its oxyd be red or blue, whilst another may be yellow and yet have for its base a red oxyd: this is precisely the case with all the yellow salts of iron. Nevertheless, besides the green oxyd and the red oxyd, there exists another oxyd of iron, which performs a very important part in the ferruginous saline combinations; and as, on the one hand, the formation of several products which are fabricated in our work-shops, and on the other, the explanation of several phænomena which often present themselves in the arts, depend upon the existence of this oxyd I think it necessary to enter into some details respecting it.

It is formed in treating iron with most of the acids, and it is obtained by decomposing the different salts, thus produced, by potash, by soda, and by ammoniac. It is especially by consideriug the solution of iron in sulphuric acid, that we can easily demonstrate its existence. If

into

into this solution, fresh prepared, we pour alkali, a white precipitate is formed, which speedily becomes green at its surface, and, in a short time, passes even into the red *. These changes of colour, which take place in the whole mass, if it be stirred, are evidently produced by an absorption of oxygene, for, if we repeat the experiment in a flask, the air which it contains diminishes sensibly in volume, and soon extinguishes a lighted paper, both when the white oxyd has become green, and *à fortiori* when it has passed into the red.

It is this white oxyd, supersaturated with sulphuric acid, which forms a great part of the sulphate of iron of commerce. Nevertheless, besides this combination, the white oxyd can also form a much more acid sulphate, and then, instead of being of a deep green, the compound inclines to a light emerald colour. The case is the same with the green oxyd and the red oxyd of iron; each of these, by uniting with the sulphuric acid, gives rise to at least two very distinct salts. Let us now examine these six kinds of sulphate of iron.

I shall distinguish the first by the names of acidulous and acid sulphate of white iron; because the oxyd in them is colourless and because the one is much more acid than the other: the second, for the same reason, by those of acidulous and acid sulphate of green iron: and the third, also, for the same reason, I shall term neutral and acid sulphates of red iron.

* I have poured super-oxydated sulphate of iron into a great excess of caustic potash, and have constantly observed a white precipitate. I have even boiled the mixture in a retort, the neck of which was immersed in water: the oxyd became green, and even red at its surface, but the lower part was always sensibly white, though it did not contain sulphuric acid, as I have convinced myself by washing, dissolving in muriatic acid, and adding nitrate of barytes.

The

The acidulous sulphate of white iron is obtained by boiling sulphuric acid, diluted with water, upon an excess of iron filings, or scrapings. Dissolved, and especially crystallized, it is always of a bottle green colour, and it is even held in the higher estimation in commerce, the more highly it is coloured. It immediately loses this colour upon the addition of sulphuric acid; it assumes an emerald green, and becomes acid sulphate of white iron, which alters the blue vegetable colours much more than the first; * it is then less fit for the operations of the arts, and accordingly the manufacturers reject it and give to the first a preference which does not depend upon prejudice, but upon the nature of the saline compounds; it may however be converted into acidulous sulphate by heating it with iron; this is what is done in several vitriol-manufactories, particularly at Beauvais, where this salt is obtained from pyritous turfs which are made to effloresce, and which otherwise would give acid sulphate, a subtance much less ad-

* When we pour sulphuric acid into a solution of acidulous sulphate of white iron, evaporated so as to mark 36° of the hydrometer, an abundant white and crystalline precipitate is almost immediately formed, which is nothing else than acid sulphate of white iron. This is the reason why it sometimes happens in the vitriol-manufactories, that the liquor suddenly becomes to a certain degree turbid, and deposits a white matter which some manufactories know and reject under the name of magnesia. It is that the solution is then too acid, and the division of this excess of acid takes place instantaneously, in such a manner that there is formed an acid sulphate which is precipitated, and an acidulous sulphate which remains in solution and crystallizes much more regularly than the first. This inconvenience might be prevented by boiling the solution longer with iron, and adding water, if this should be necessary; for the acidulous sulphate of white iron, or the sulphate of iron of commerce, when too much evaporated, is also susceptible of concreting in part, or at least suddenly in a mass.

vantageous

vantageous to be introduced into commerce. The same may be done with every kind of acid sulphate, whatever be its origin; whether it be artificial or natural the transformation always takes place: it would also take place even if the oxyd of iron were green or red. The acidulous and acid sulphates of white iron are both thrown down in a white precipitate by the alkalies; they suddenly decompose the oxygenated muriatic acid, and pass, according as more or less is added, into the state of sulphate, in which the oxyd is green or red. It is in this manner that the oxygenated muriatic acid acts upon the pure white oxyd; such is also the action of the air upon the combinations of this oxyd, with the acids, and especially with the sulphuric acid. Hence we see why the colour of its solutions is not constant; from the green they pass into the red; the liquor becomes turbid, deposits a yellow matter, and then ceases to be coloured. All these phenomena may be naturally explained, and are only consequences of the properties presented to us by the other sulphates of iron of which we shall proceed to treat.

TO BE CONCLUDED IN OUR NEXT.

List of Patents for Inventions, &c.

(Continued from Page 320.)

WILLIAM SAMPSON, of Liverpool, in the county of Lancaster, Wheelwright; for certain improvements in the application of power employed mechanically, especially as adapted to the use of cranks and fly-wheels, or other contrivances producing equivalent or similar effects. Dated February 12, 1806.

JOHN

JOHN PHILLIPS, of East Stone-house, in the county of Devon, Stone-mason and Sculptor; for certain improvements in the construction of tinder-boxes. Dated February 12, 1806.

JOHN PHILLIPS, of East Stone-house, in the county of Devon, Stone-mason and Sculptor; for a chain and apparatus for straight, square, and parallel stone and marble sawing; which chain may be applied to other useful purposes. Dated February 12, 1806.

JOHN MARSHALL, of Northwick, in the county of Chester, Salt Proprietor, and JOHN NAYLOR, of the same county, Salt Proprietor; for a new and improved method or manner of manufacturing and making salt. Dated February 14, 1806.

THOMAS KENTISH, of Baker-street, North, in the parish of Saint Mary-le-bone, in the county of Middlesex, Esquire; for certain improvements in the construction of machines or engines applicable to the moving, raising, or lowering of heavy bodies and weights of all kinds, either upon land or on board of ships and vessels. Dated February 20, 1805.

JOHN JONES, the younger, of Birmingham, in the county of Warwick, Tool-maker and Die-sinker; for improvements in the mode of manufacturing barrels for fire-arms. Dated February 20, 1806.

JOHN WOODHOUSE, of the parish of Heyford, in the county of Northampton, Engineer; for certain improvements relative to canals. Dated February 20, 1806.

THE

REPERTORY

OF

ARTS, MANUFACTURES,

AND

AGRICULTURE.

No. XLVIII. SECOND SERIES. May 1806.

Specification of the Patent granted to RICHARD BROWN, *of the Parish of Saint Botolph, Bishopsgate, in the City of London, Cabinet-maker; for certain Improvements in the Construction of several Parts of Tables, and of various other Articles of Household Furniture which stand upon or are supported by Legs or Feet.*

Dated November 26, 1805.

With a Plate.

TO all to whom these presents shall come, &c. Now KNOW YE, that in compliance with the said proviso, I the said Richard Brown do hereby describe and ascertain the nature of my said invention, and the manner in which the same is to be performed, as follows; that is to say: In dining and other tables, or furniture of similar description and use, I do construct, connect, and form together, my top rails or framing, or bed, or that which immediately supports the platforms, boards, or leaves of the same, by whatsoever name or names such rails or framing, or bed, may be known or denominated, in such a manner as that the same may be disposed or folded up

in a small space when not required to be used. And I do declare that the annexed drawings and descriptions do sufficiently exhibit and explain the same.

Fig. 1 (Plate XV.) represents the upper surface of frame when shut up or folded together.

Fig. 2 represents the upper surface of table-frame when the said frame is extended. A to B extending division of table-frame: the construction and manner of operation of which must be obvious on inspecting the drawings, and which may be repeated to any desired number or length. The pieces of which these extending divisions are composed are connected together by means of joints. C and D the advancing rails to which the blocks G H I K are fixed. G H I K blocks to which the end flaps are hinged, and from which the said flaps are suspended when the brackets L M N O are put back against the advancing rails. P Q R S vacancies made in the brackets L M N O to receive the blocks G H I K when the said brackets are put back against the advancing rail. The end flaps being connected with the advancing rails by means of the blocks and hinges, may be moved forward or backward with the said advancing rails; and when the said end flaps are raised up they may be supported by the brackets turned forward from the advancing rails for that purpose. To the piece marked T a leaf or board may be fastened, and legs or other support or supports may be fixed to the advancing rails, to the brackets, or to other parts of the frame. And I do farther declare, that the joints in the said framing, and the methods of connecting the pieces together, may be made and adapted according to any well-known and good form; and that the number and disposition of the pieces, as well as the materials of the joints themselves, whether of wood, metal, or other fit substance, and

the

the form and situation of the legs, are susceptible of considerable variations, according to the uses and purposes to which the frames are to be applied, or the judgment of the maker, or the taste or choice of the buyer.

Fig. 3 represents the frame-work of a table in perspective of a different structure from that of Figs. 1 and 2, still retaining the same general characters which distinguish my said improvement as before described. When the boards of the tables are all respectively laid in their places upon the expanded frame, and keyed, bolted, forked, or otherwise sufficiently well and firmly secured in their respective situations, they constitute one piece, which gives steadiness and firmness to the whole structure.

And I do hereby farther declare, that another of the improvements which constitute part of my said invention, is exhibited and explained in the annexed drawings, Figs. 4 and 5, of an instrument for connecting together two brackets, placed on the opposite sides of a block or frame, so that if either of them be moved outwards on one side, the other shall move outwards on the opposite side at the same time, and if either be moved inwards, the other shall move inwards at the same time.

Fig. 4 represents the surface of the block or frame with the brackets when put back against its opposite sides. A is the connecting instrument made of iron or brass, or of any other suitable material or materials, jointed at B and C. D is a centre pin, fixed in the block on which the instrument is moved. E and F are pins fixed to the brackets; on which pins the ends of the instrument move. It is obvious, from Figs. 4 and 5, that when the handle is drawn out the connecting instrument will alter its form, as in Fig. 5, and both brackets will be carried out together. It is also evident, that legs or supports being fixed

to the brackets, will be moved backward or forward with them, and that legs or supports may be fixed to the block or frame in various situations, according to the purposes to which the said block or frame may be applied.

And I do hereby farther declare, that another of the improvements which constitute part of my said invention is exhibited and explained in the annexed drawings, Figs. 6 and 7, which represent an instrument for connecting together two brackets, (placed in the situation shewn by Fig. 6,) so that, if either of them be moved forward, the other shall move forward at the same time; and if either be moved back, the other must necessarily go back at the same time.

Fig. 6, A is the lower surface of a card-table frame. B C are the two brackets. D is the joint on which the two brackets move. E is the connecting instrument, made of iron or brass, or of any suitable material or materials. H and I are pins fixed in the brackets. F and G are pins fixed in card-table frame, for the purpose of guiding the connecting instrument in a strait direction, when the said connecting instrument is moved forward or backward.

Fig. 7 shews the situation of the connecting instrument E and of the bracket B, when the other bracket C is moved forward. It is obvious that the connecting instruments before described, Figs. 4, 5, 6, 7, may be applied to the brackets of various articles of household furniture, and that legs or supports may be fixed to the said brackets if required.

In witness whereof, &c.

Note inserted on the Drawing. H a strong iron plate, which being screwed to the pieces I and K, joins together the said pieces I and K in such a manner that they form a single long arm.

Specification

Pl. XV. Vol. VIII. Second Series.

Fig. 2.

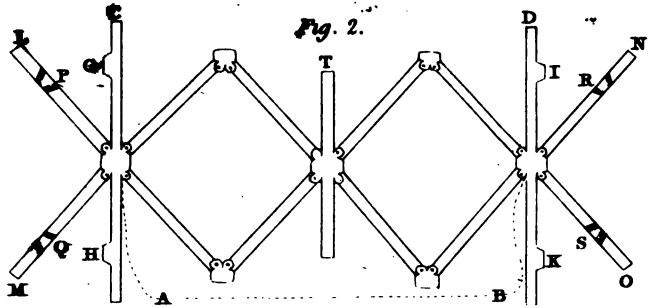
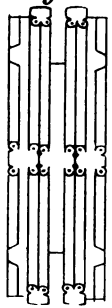


Fig. 1.



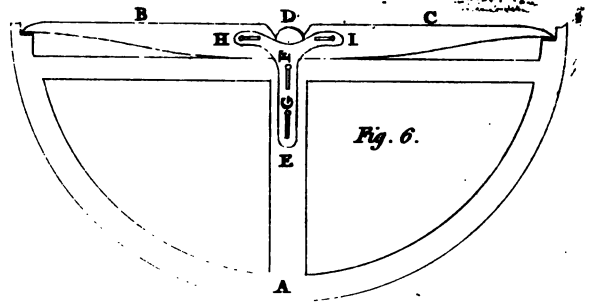
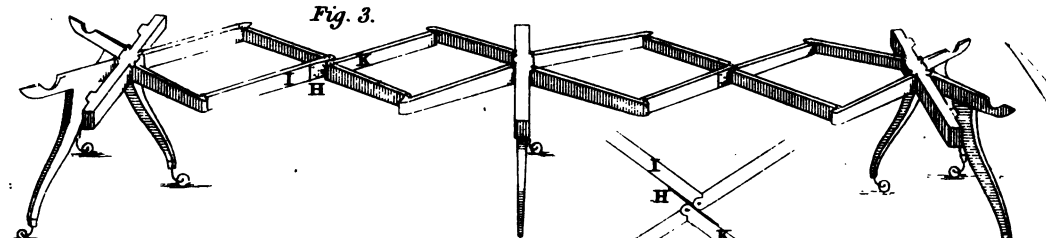


Fig. 6.

Fig. 3.



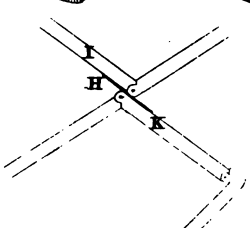


Fig. 7.

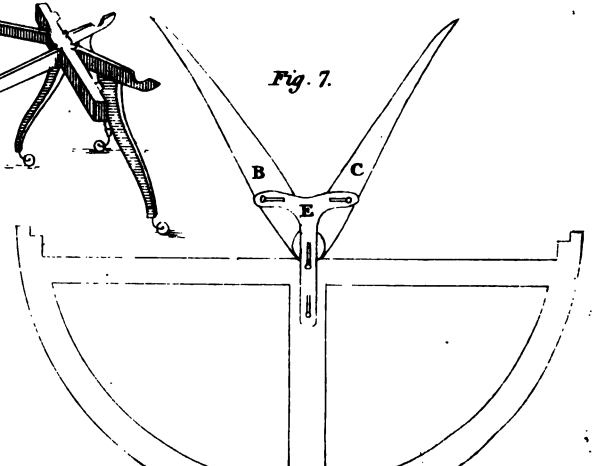


Fig. 4.

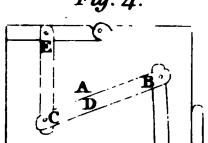
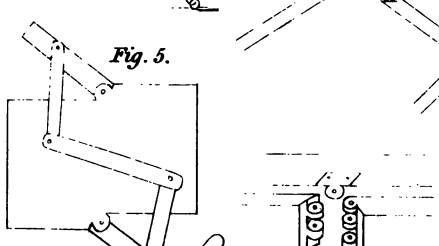


Fig. 5.



Specification of the Patent granted to JOHN WOODHOUSE, *of the Parish of Heyford, in the County of Northampton, Engineer; for certain Improvements relative to Canals.* Dated February 20, 1806.

TO all to whom these presents shall come, &c. Now KNOW YE, that in compliance with the said proviso, I the said John Woodhouse do hereby declare that my said improvements relative to canals are described in manner following, and comprised under the four following heads, *viz.* first, the application of certain contrivances for weighing boats, barges, or other vessels, either loaded or unloaded; secondly, in an improved method of conveying boats, barges, or other vessels, from one level of a canal to another without the use of locks; thirdly, for the application of a signal or telegraph, to enable the lock-keepers to inform each other when any boat, barge, or other vessel is approaching the locks, whereby very considerable quantities of water may be saved; fourthly, in a new method of raising boats, barges, or other vessels that are sunk.

First. As to the application of certain contrivances for weighing boats, barges, or other vessels, I cause an horizontal frame of timber to be erected over a lock, of a sufficient height above the surface of the canal to admit the boats, barges, or other vessels to pass under it, and of competent strength to support the weight of the boats, barges, or other vessels, with their cargo, intended to be weighed. Upon this frame or platform place as many weighing machines, of the best construction, as are applicable to this purpose, and capable of sustaining and weighing the said boats, barges, or other vessels. Let the weighing machines be so arranged on the frame or platform

platform, that chains or bars, depending from the short ends of the levers, may form two parallel rows, at such a distance asunder as to admit the boat or boats, barge, or other vessel or vessels, intended to be weighed, to pass between them, and in length somewhat shorter than the boats, barges, or other vessels. Across the bottom of the lock place as many pieces of timber or iron as there are pairs of chains or bars depending from the levers of the weighing machines. If these cross pieces be timber, they must be loaded with metal, so as just to sink in water. To each end of these cross pieces a strong chain must be fastened; and each of the chains or bars depending from the machines must terminate in a strong hook, and be furnished with an adjusting screw or wedge capable of lengthening or shortening the bars or chains. When a boat, barge, or other vessel is intended to be weighed, it must be made to swim into the lock, and the cross pieces drawn up by their chains until they come into contact with the bottom of the boat, barge, or other vessel. The chains of the cross pieces are then to be hooked to the depending bars or chains, and to be made equally tight by the adjusting screws or wedges. A sufficient quantity of water is then to be let out of the lock into a side pond, (and when the lock is empty to be let into the lock again, by which means no water is lost,) to leave the boat or boats, barge, or other vessel or vessels, suspended by means of the cross pieces and chains to the weighing machines. Now, in order to ascertain the whole weight sustained, it is only necessary to connect the main levers of all the machines together by means of a bar or bars, or other obvious means, parallel to their centres; and a weight or weights suspended from this bar or bars will act at the same time on all the machines, and indicate the weight of the boat or boats, barge, or other

other vessel or vessels, and the cargo. It will be proper to erect a roof over this apparatus to protect it from being injured by the weather. When it is expedient to erect an apparatus of this description where there are no locks, a side lock may be made for the purpose, and the water drawn out of it so as just to leave the boat, barge, or other vessel or vessels, suspended; or the boat, barge, or other vessel or vessels, may be lifted out of the water by any other well-known mechanical means, and suspended underneath the machines as before described.

Secondly. My improved method of conveying boats, barges, or other vessels, from one level of a canal to another without locks, is described as follows. The upper and lower levels of the canal are to be brought to within such a distance of each other as shall be somewhat more than the length of the boat, barge, or other vessel or vessels intended to be conveyed. Each of the levels are to terminate in two canals, each wide enough to admit the boat, barge, or other vessel or vessels. The space between the two levels must be divided lengthwise into two spaces by a wall or partition of timber, of a sufficient strength, and carried up with the ends and side walls a sufficient height above the top level, to fix the machinery upon, turning a proper arch or arches in the end-wall next the lower canal, for the boat, barge, or other vessel or vessels to swim underneath. Each of these spaces must be sufficiently large to admit a water-tight vessel, which I call a conductor, capacious enough to swim the boat, barge, or other vessel or vessels in. Each of these conductors must be furnished with a stop-gate or paddle at each end; and the ends of the upper and lower canals must also have stop-gates or paddles. The two conductors must be suspended by a competent number of ropes or chains, one end of each to be made fast to strong pieces

pieces of iron or timber fastened to the sides of the conductors, and meeting over the centre; and the other ends fastened to two drums or wheels upon two horizontal shafts, (I prefer the flat rope used for drawing coals,) each drum and shaft should be in several lengths, connected together so as to form a sufficient length, somewhat more than the length of the conductor, and to be of a sufficient strength more than equal to the weight of the conductor when filled with water. These drums must be of equal diameters, and placed horizontally over the conductors parallel to each other. Every other of the ropes fastened to each conductor must pass over the drum that is over the said conductor, and be fastened to the other drum: for example, if there be for a boat of thirty tons burthen thirty ropes to each conductor, each rope capable of sustaining the weight of five tons, fifteen of those ropes belonging to each conductor will be fastened to each drum, and the other fifteen ropes of each conductor will pass over the drum over each conductor respectively. The ropes must be of such a length, that when one of the conductors is opposite the upper level, the other conductor will be opposite the lower level. The drums must be so placed, that when the ropes are wrapped round them the centre of all the ropes wrapped upon each other round the drum shall be over the centre of the conductor, (when joined to the upper canal,) and the centre of the lower canals must be under the centre of the first wrap of the rope round the drum. A counter balance to the weight of the ropes is effected by their coiling on the drums, and the height of the lift being given, it is easy to find the diameter of the drums to produce this effect. Or one drum formed of several parts equal to the length required, whose centre is over the centre of the partition wall, and of such a diameter

as

as to extend nearly over the centres of the conductors; or the drum may be made smaller, and fixed over the centre of the dividing wall, and small drums, pullies, or rollers, fixed so that the ropes may descend perpendicularly over the centre of the spaces in which the conductors rise and fall alternately. The ends of the canals must be truely made, and covered with leather, which must be stuffed between the leather and wood, to form an elastic body; so that when the ends of the conductors are forced against them, by a spring or any other contrivance, they may be water-tight. To pass a boat, barge, or other vessel, from one level to another, for example, from the lower level to the upper level, open the gate or paddle in the lower conductor, and the corresponding gate or paddle in the lower canal, and swim the boat, barge, or other vessel into the conductor, which will displace a quantity of water from the conductor equal in weight to the weight of the vessel and cargo, so that the conductor, with its contents, is exactly of the same weight, whether the boat, barge, or other vessel is in it or not. When the vessel is in the conductor, and the gates or paddles shut, the apparatus is to be set in motion by a pinion acting in a wheel fixed on the axis of the drum, or by any other mechanical contrivance; and the top conductor being with the water in it equal to the weight of the lower one, will descend, and the bottom conductor, with the vessel in it, will rise; when it arrives at the upper level, the top conductor will have descended to the lower level. It is obvious that one vessel may be lowered in one conductor while another is rising in the other conductor, since the equilibrium is not destroyed by the vessel entering the conductor. It may be expedient to give the descending conductor more weight than

the ascending one, to produce motion in the apparatus with more ease, which may be effected by not suffering the descending conductor to go quite so low as to bring the surface of the water in it to the level of the water in the lower canal, so that when the paddles are opened a small quantity of water will run out of the conductor into the lower canal. To stop or prevent this apparatus from moving with two much velocity, a common break may be applied upon a wheel fixed upon the axis of the drum, or any other contrivance which will produce the same effect. The strength of this apparatus, and number of ropes, will depend upon the weight of the vessel intended to be conveyed. Grooves should be made at a small distance from the paddles at the ends of the canal, and a spare paddle or gate kept, in order that the paddles and grooves which are constantly to act may be kept clean, and in good repair. It is necessary to keep the spaces into which the conductors descend empty, which may be effected by means of a culvert, or by the application of a pump.

Thirdly. My application of the telegraph or signal to the purposes of canal navigation will produce a very considerable saving of water in passing locks, when they are so far distant from each other that the lock-keepers cannot see the boats from one lock to another; for it takes no more water to pass a given number of boats, ten for example, up the locks, and as many down, provided they pass alternately, than it would to pass ten boats in succession, in either direction, by the assistance of the telegraph or signal. The telegraph or signal may be a strait piece of timber, (of height sufficient to be seen by the next lock-keeper,) with a board framed into the upper end of it about eighteen inches long and one

foot

foot broad, having a round hole cut through it about eight inches in diameter, a frame being fixed in the ground to receive this piece of timber when raised perpendicular, and in which frame it will turn round; therefore, when the first lock-keeper has a boat in view upon the canal he turns the flat side of the board towards the next lock, which informs the next lock-keeper that there is a boat coming in that direction: the middle lock-keepers are furnished with two telegraphs or signals, to give information each way. By this communication the lock-keepers always know if there are any boats near, by which means a great deal of water may be saved. Any other kind of signal or telegraph may be applied to this purpose, but the above appears to me the simplest.

Fourthly, My method of raising a boat, barge, or other sunken vessel, is as follows. I moor two loaded boats along-side of the sunken vessel, with two or more pieces of timber, long enough to project over each side of the loaded boats half the breadth of a boat, with a pulley or roller fixed at each end of the timbers, for one or more ropes or chains to pass over; one end of the ropes or chains fastened to the boat sunk, and the other end to an empty boat, on the outside of each of the loaded boats. When all these ropes or chains are tightened or made fast, by unloading the loaded boats into the empty ones, the sunken boat will thereby be raised.

In witness whereof, &c.

Specification of the Patent granted to JOHN JONES, *the younger, of Birmingham, in the County of Warwick, Tool-maker and Die-sinker; for Improvements in the Mode of manufacturing Barrels for Fire-Arms.*

Dated February 20, 1806.

With a Plate.

TO all to whom these presents shall come, &c. NOW KNOW YE, that in compliance with the said proviso, I the said John Jones do hereby declare, that the following is a particular description of the nature of my said invention, and method whereby the same is to be performed. It is necessary to be observed, that the barrels commonly called twisted barrels are formed by winding a small square or flatted bar of iron spirally round a mandril, while the said bar is in a red-hot state; and then withdrawing the mandril, and heating the bar to a welding heat, it is jumped or knocked together in the direction of the length of the mandril, which causes the spires or twists of the bar, or skelp as it is commonly called, to cohere together, and to form a twisted barrel or tube. Other welding heats are then given to it, and it is forged upon a mandril to the proper form and thickness. This method is defective in as much as the joints of one part of the skelp to the other are necessarily at right angles to the mandril, and have not proper hold on one another; so far is the method generally practised in common use. My newly-invented method consists in forming the bar or skelp so as to be gradually made thinner from one end to the other, and also in bevelling or thinning it at each edge, so that when cold or twisted upon the mandril the edges considerably overlap each other, and then withdrawing the mandril, and taking a welding heat upon the skelp, another mandril is inserted,

inserted, and the skelp forged upon it at one or more heats, as may be convenient. As for the process of welding and forging I claim no exclusive privilege, but merely for the mode of producing the form of the skelp, and the method of twisting or coiling it.

The drawing, marked bar No. 1, (Plate XVI.) represents a bar of iron or steel, of any required length or breadth; which bar is passed, when hot, between the rollers A B of the machine, represented in Fig. 1, the rollers having grooves turned upon them, to cause the bar No. 1 to be bevelled on each contrary side, as is represented in the drawing of the bar No. 2, and the section of the same No. 3. The bar No. 2 being again passed between the rollers A and B, for the purpose of working it taper in thickness from C to D; and in some cases it may be necessary to heat the bar more than once, and to pass it between the rollers A and B more than twice. A method of increasing and decreasing the pressure of the rollers is shewn by the machinery E F, &c. at the top of the machine, Fig. 1. But any other mechanical means may be adopted for the purpose, at the discretion of the mechanic who may prefer them. The skelp or bar, being now bevelled and tapered, is put into an air furnace, as at Fig. 2, of which Q is the draught plate and R the fire. At the mouth of this air furnace is fixed a small pair of rollers, as in Fig. 3. When the skelp is hot enough, the end nearest the mouth of the furnace is brought between the small rollers at S, whose forms are represented in the small drawing No. 4, one roller being a little concave, and the other convex, as being adapted to guide the bar or skelp to the twisting mandril U. This twisting mandril U is conducted and moved, and impelled forward, or drawn backward, by the action of the machine, as represented in Fig. 4.

This

This machine is for the purpose of twisting or winding the hot bar or skelp round the mandril U, and is performed as follows. The pinion wheel W is moved by the power of steam, water, horse or horses, man or men, or any other sufficient power, and gives action to the main wheel X, whose shaft Y has a square perforation, sufficiently large to admit the directing screw Fig. 5 to pass through; the worm of which is cut of a necessary size consistent with the width of the skelp or bar. The directing screw is supported and moves in a coupling box, as at *a a*, which causes it to recede or advance whichever way the wheel X turns. The twisting mandril U is attached to the directing screw *b b* at C; and for one revolution of the screw there is of course one of the mandril U; consequently when that part of the mandril U, as at *d d*, is opposite the rollers S, it is in its convenient situation to receive one end of the bevel bar or skelp V; which being held by a fork or catch *e*, is compelled, while the machine, Fig. 4, is in action, to wrap, coil, or twist round the said mandril U to any given length, according to the purpose required. The lever *f* may be occasionally used to press the roller S tighter upon the skelp or bar V, in order that it may wind more regularly round the mandril U. Before the spiral skeleton No. 6 is taken off the mandril U, it should be hammered, in order to loosen it from the mandril, and close its joints; it should then be welded. The section No. 7, with the mandril, represented within it, shews the manner in which one bevel edge over-laps the other.

I do hereby farther declare, that such barrels may be made in any metal, or composition of metals, that may be found sufficiently strong and proper for the purpose.

In witness whereof, &c.

Specification

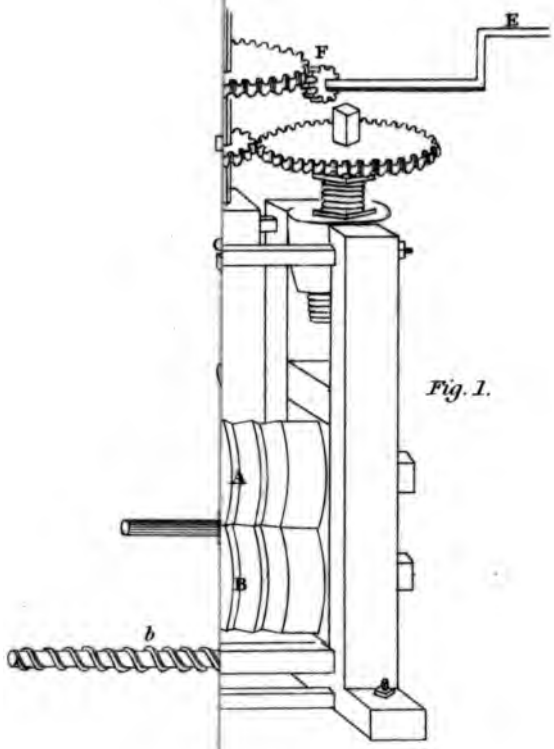
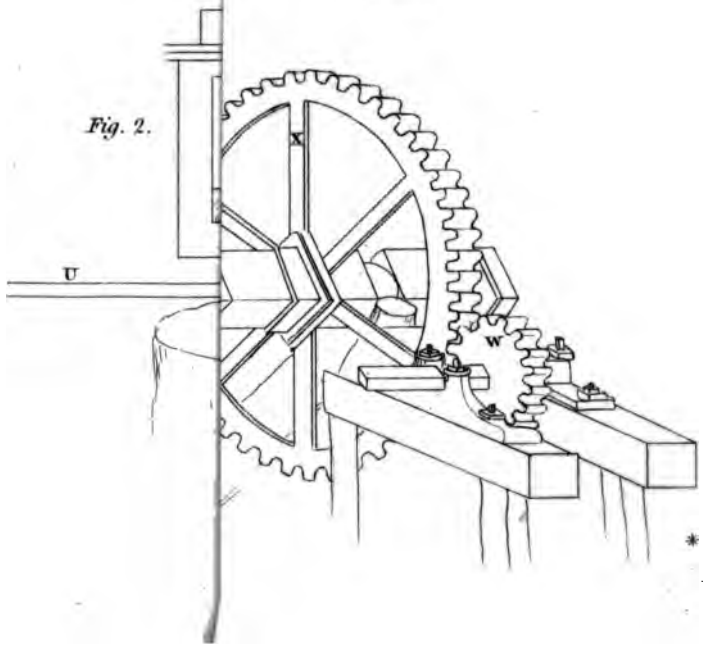
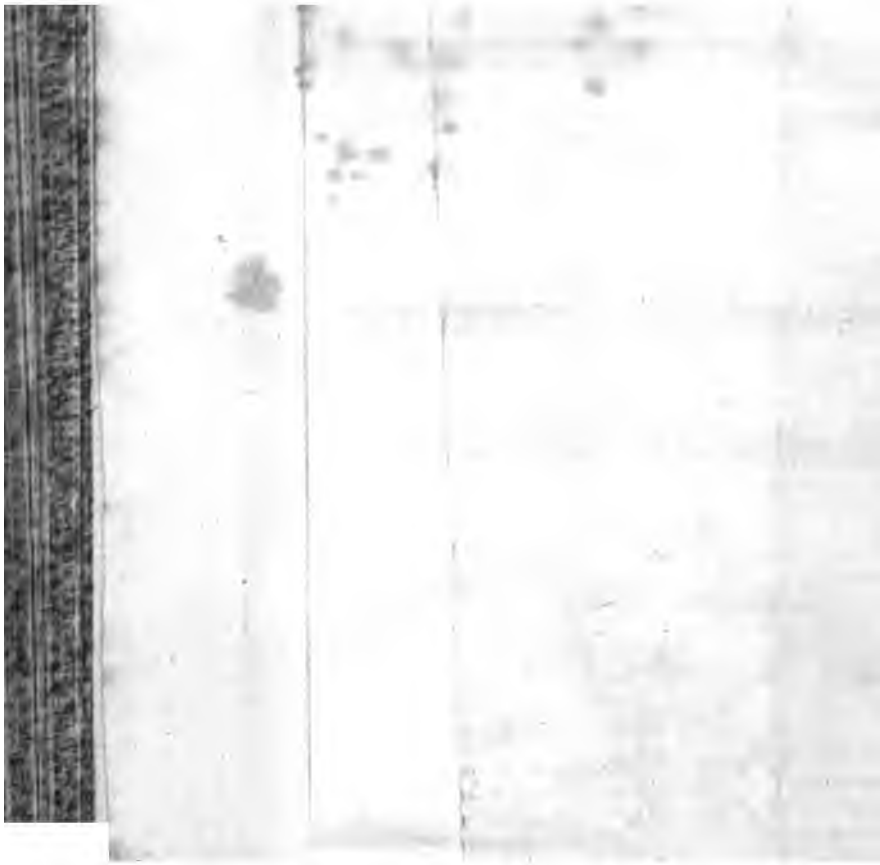


Fig. 1.

Fig. 2.





Specification of the Patent granted to MARC ISAMBARD BRUNEL, *late of Gerard-street, Soho, but now of Queen-square Place, Westminster, in the County of Middlesex, Gentleman; for new Trimmings and Borders of Muslin, Lawn, and Cambric.* Dated November 25, 1803.

TO all to whom these presents shall come, &c. NOW KNOW YE, that I the said Marc Isambard Brunel, in compliance with the said proviso, do hereby declare that my invention consists in weaving narrow webs of muslins, of cambric muslins, of lawns, or of cambrics, with a proper selvage at each edge, varied according to the purpose for which the article is wanted, but in all cases adapted to prevent its ravelling out in washing, and of any breadth not exceeding ten inches; so as that these narrow webs shall, without hemming, whipping, or otherwise securing the edge by needlework, be ready to be used for the different kinds of trimming required to be made of the materials above specified. By trimmings to be made of the materials above specified, I mean frills, borders, trimmings for bonnets, caps, cloaks, or for any other article of dress, or even of furniture, provided the trimmings are muslin, cambric muslin, lawn, or cambric, and not exceeding ten inches in width. The advantage of this invention, beside that the trimmings produced are in many cases neater than the trimmings of which the edges are secured from ravelling by needle-work, is, that the operations of hemming, whipping, or otherwise securing from ravelling, the edges of trimmings cut in narrow slips out of broader webs, as they have unavoidably been hitherto, are by this invention altogether saved. In cases where trimmings of the materials above specified are required to be gathered, or

to

to be whipped, for the purpose of being drawn up full, I introduce into the warp a strong or double thread, or threads, of cotton, flax, or silk, on which the trimming may be drawn or gathered up without the need of employing any needle-work for this purpose. The trimmings may be woven either in looms similar to those in use for weaving muslins, cambric-muslins, lawns, or cambrics, according to the kind of trimming, with the only difference, that the looms for these trimmings must be proportioned to the narrowness of the web required, or these trimmings may be woven in ribband looms, and very advantageously by the engine looms, such as are used by ribband-weavers, with the precaution of using such sizing, starching, and other dressing of the yarn, as is proper for weaving flaxen or cotton webs, as the case may be, of the usual breadth, and of the same quality respectively. These trimmings may be woven with plain selvages, so as not to shew any appearance of hem, roll, or turning-in; or they may be woven with a thick stripe at one or both edges, so as to give the appearance of a hem, as well as to give additional strength to the edge or edges. The edge or edges of these trimmings may be woven double, so as to imitate a hem more exactly, and so that a quill, bodkin, or wire, might be drawn through the hollow part, as is sometimes customary in regard to the broad hems made to trimmings worn in mourning; and trimmings in which this double part is required may be woven in looms of a construction similar to those in which ribbands with hollow edges are woven, as is the case in some of those woven for hat-bands. But when it is required that each of these folds of the double part should be of the same texture with the single part of the web, it is necessary that the weft should be woven four times through the warp of the edge (that

is,

is twice for each fold) for every twice that the weft is woven through the warp of the single part of the web. In order that these trimmings may wash and wear equally well with trimmings cut crosswise out of broader webs of the same quality, yarn similar to the weft of the broad webs may be used for the warp of the web for trimmings, and yarn similar to the warp of the broad webs may be used for the wefts of the web for trimmings. These trimmings may be worn either plain, striped, checked, or figured, in white or in colours, by means similar to those practised in the weaving figured ribbands, and they may be finished by singeing, dressing, bleaching, printing, calendering, clear-starching, or by any other of the operations used in finishing webs of the usual breadth, according to the nature of the materials, and the purpose to which the trimmings are to be applied.

In witness whereof, &c.

Description of a Cheap Bedstead for a Workhouse, Hospital, or any of his Majesty's Barracks; Fever Ward, &c. &c.

Communicated by Mr. MATTHEW GREGSON, *of Liverpool, in a Letter to the Editors.*

With an Engraving.

A A, Fig. 1, (Plate XVII.) are two cast iron dogs, of the dimensions described B.

B is another view of the dog, with the dimensions.

C is one of the middle ribs, $4\frac{3}{4}$ inches broad, $1\frac{1}{2}$ thick, (when worked).

D D are the two outside ribs, 7 inches broad, same thickness.

F is the head-board, which lifts off at pleasure, and is fixed by a rib fastened behind, which falls into two holes in the side pinions D D.

All the ribs of one hundred bedsteads, marked E E E, are exactly alike, and all the dogs cast in the same mould; the like care is made in making the side pieces D D, which are made stronger, to sit upon occasionally; so that admitting the head-board F is fitted, and the holes bored by one gage, which is easily done, one hundred bedsteads (or five hundred bedsteads,) may be taken down, washed, aired, or drest for vermin, if need be, and put up again in as short a space of time as possibly can be conceived, by any person of a most ordinary capacity, without hammer, screw-driver, or other implement whatever.

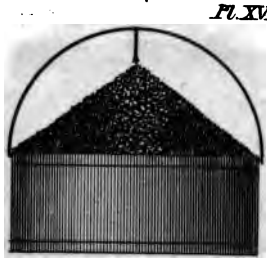
The above bedsteads, which for cheapness and simplicity of construction, will be found superior to any I have seen, or to any in general use; I have had the honour of superintending, in our new house of recovery, adjoining to the parish workhouse, in Liverpool, where they are introduced; but I do not claim all the invention, for the first hint of this construction I received at the Castle of Chester, from some made by and under the orders of that ingenious architect Mr. Harrison, of classical celebrity.

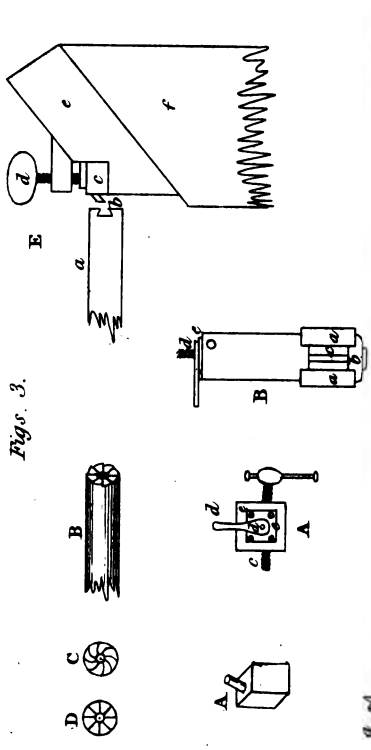
If this sketch meets your approbation, you have liberty, through the pages of your useful work, to make it known to the public, for whose benefit it was invented, and for whose service it is now offered. It may be useful at lazerettos, &c.

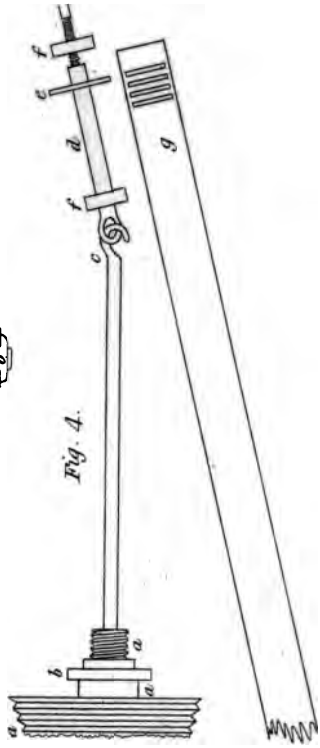
Liverpool, Yours, &c.
Feb. 24, 1806. MATTHEW GREGSON.

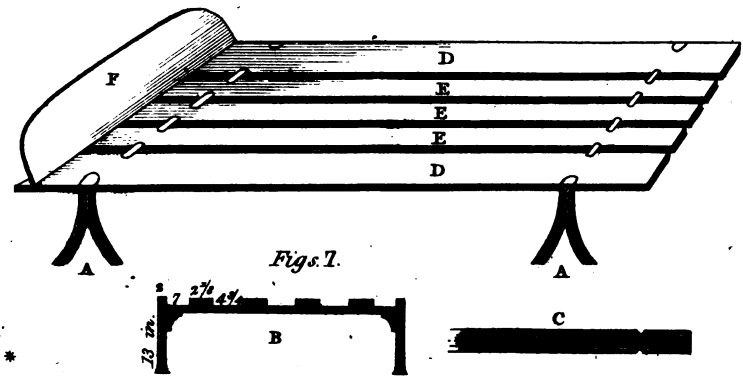
Account

Fig. 2.









Figs. 7.

Account of the Regulation proposed to Parliament by Mr. ROBERT VAZIE, *of Rotherhithe, to prevent Frauds in the Measurement of Coals.*

Communicated by him in a Letter to the Editors.

With an Engraving.

GENTLEMEN,

THE various impositions to which the consumers of coals in the metropolis were subject, in the sale, admeasurement, and delivery of that article, so necessary to the comforts of life, and so important in manufactures, attracted, in the year 1800, the attention of the Legislature, which referred the consideration of the state of the trade to a Committee of the House of Commons; who were pleased to report their opinion, "that parliamentary regulations and restrictions were necessary to be adopted therein." Since that time the Corporation of the City of London has procured a controul over the sale by an Act of Parliament, which extends to the delivery of coals; but the defects in the former acts respecting *the admeasurement* continue as heretofore.

The Committee of the House of Commons having honoured me with an approbation of my improvement of the coal-measure, I have presented a bill to Parliament this session to establish the same for the admeasurement of all sea-coals upon which duties are chargeable.

If the insertion of the annexed observations on that subject, with a description of the coal-measure, be consistent with the design of the Repertory of Arts, &c. I shall esteem myself much obliged by their publication.

Rotherhithe,
March 24, 1806.

Yours, &c.

ROBERT VAZIE.

Description of the Coal-Measure proposed.

Upon the outside of the legal bushel, or other coal-measure, is placed a moveable iron bow, of a semi-circular form, (much like the bale or handle of a pail,) from the middle of which is suspended, on a loose joint, a pin, which forms a gauge. (See Fig. 2, Plate XVII.) At the time of filling the measure, the bow and gauge lie on the outside of the bushel; when it is filled, the base of the heap is circumscribed and accurately defined by the bow, and the just height is marked by coming in contact with the lower end of the gauge.

Observations.

In the 16th and 17th years of the reign of King Charles the Second, an Act was passed, directing, "That all coals brought into the River Thames, and sold by the chaldron, shall be at the rate of thirty-six bushels *heaped up*, according to the bushel-measure sealed for that purpose at Guildhall."

And in the 12th year of the reign of Queen Anne, an Act was passed, directing "That the coal-bushel shall be round, with an even bottom, and be nineteen inches and a half from outside to outside, and shall contain one Winchester bushel and one quart of water; and all coals chargeable with duties shall be computed by the chaldron, containing thirty-six bushels *heaped up*; and that a standard measure shall be made, and kept in the Exchequer."

And in the 43d year of the reign of his present Majesty, an Act was passed, directing, "That the bushel shall be *heaped up* in the *form of a cone*, the outside of the measure being the extremity of the base thereof."

The descriptions contained in the above Acts are fully explanatory, so far as they respect the form and dimensions of the coal-bushel, and the base of the heap; but neither

neither the former expression, "*heaped up*," nor the latter, "*in the form of a cone*," establishes a criterion to ascertain the height of the heap; any elevation, from four to eight inches above the measure, is equally conformable to those descriptions. To this defect is to be ascribed the late nefarious practices in the admeasurement of coals, which, for a series of years, rendered the various laws made to protect the consumer perfectly nugatory: but this evil has of late been considerably remedied, in wharf-measure, by the introduction of my invention of a moveable *bow-gauge* placed near the top of the established bushel, whereby the *height of the heap is defined with accuracy and dispatch.*

This improvement of the measure has been invariably used by order of the principal land coal meters for the city of London and county of Surrey, in the re-admeasurement of coals, for upwards of five years; and has, upon different trials in law, received the sanction and approbation of the Courts in the strongest terms, as ascertaining, with the greatest precision, the quantity of coals admeasured; for want of which, formerly, actions for a deficient delivery had generally failed in proof. Notwithstanding these instances, experience shews that the practice can only be rendered *general* by the authority of the Legislature.

The elevation of the heap upon the bushel is now fixed, by the major part of the principal land coal meters, at seven inches, because at any less height the cone cannot without great delay be regularly formed; but the meters can at present *alter the height of the heap* at pleasure—a power with which few individuals can safely be trusted.

In the Port of London coals are delivered from on board of ship with a vat, or measure containing nine bushels, which from its extended diameter, admits of greater

greater variation in the *height* of the heap than the bushel itself.

Another evil exists in the admeasurement of coals in the cellars of the retail dealers: these coals are usually carried out by porters; and the purchasers, who are chiefly persons in the lower situations of life, have no prompt means of redress in case the coals shall be delivered according to strike-measure, although they have been charged a price equal to the heaped measure; and the difference in quantity is nearly one-third.

As the honourable the Committee of the House of Commons were pleased to recommend that some parliamentary regulation should be adopted in the measurement of coals; a Bill has been presented to Parliament to cause the *bow-gauge* to be fixed, conformably to the models submitted to the said Committee, upon all measures used in the measurement of coals chargeable with duties; and if the said Bill shall be passed into a law, it will effectually prevent those fraudulent practices which have hitherto proved so injurious to the revenue, the ship owner, the respectable merchant, and the consumer of coals.

Extracts from the Committee of the House of Commons, appointed to enquire into the State of the Coal Trade in the Year 1800.

"Mr. Richard Austin Vaux, Deputy Sea-Coal Meter.

"Is there a way of filling the vat so as to hold a larger or smaller quantity of coals?"

"There is: it depends upon the quality of coals, and the attention of the officer who inspects the vat. By the present mode of working, nothing but the eye can guide you, as it depends upon the height of the coals in the vat."

"Mr.

"Mr. John Nettlefold, Deputy Sea-Coal Meter.

"Is there any difference between measuring by the bushel by the land meters from what is practised by the ship meters who use the vat?"

"The land meters give little more than strike measure, whereas the ship meters give heap measure."

"Mr. Edward Parish, Principal Land Coal Meter for the City of London, delivered in instances of various frauds in the measure of coals.

"Can you suggest any Improvement in the admeasurement of coals by which more certainty could be produced?"

"I have seen a proposition of Mr. Vazie's on this subject, which I very much approve of; it is a bushel measure, with a sweep or handle to ascertain the heap."

"Mr. John Burnett, Principal Land Coal Meter for the County of Surrey, delivered in various instances of frauds in the measure of coals?"

"Can you suggest any regulations which would have the effect of preventing frauds in the admeasurement and delivery of coals?"

"I have seen the proposed improvement by Mr. Vazie, and I think that the form proposed by him should be adopted; as it would enable a meter to deliver a cargo with much more dispatch."

"Mr. William Gibbons, Sealer of Measures for the City of London, was called in, and questioned,

"As to his knowledge of the present mode of measuring coals, and as to any means of remedying the defects and abuses therein."

"He stated, that he has been informed by a sea-coal meter, that they can put twelve (in place of nine) bushels into a vat: that he is of opinion the measure with a gauge would deprive the meter of the power of injuring either

either government, the ship owner, merchant, or consumer. As measure-maker, he is of opinion the gauge proposed by Mr. Vazie may be made sufficiently strong, without in any material degree adding to the weight, or at all injuring the measure."

"Mr. Robert Vazie was called in, and asked,

"Whether he was acquainted with the present mode of measuring coals from the ship into the barges or lighters, and whether he could point out any improvement in that business?"

"He said he was acquainted with the present mode, and produced to the Committee several models of the kind of measure at present used, and also of those which he would recommend to be adopted instead of them, together with a paper explanatory thereof, which he delivered in."

"Resolved, That it is the opinion of this Committee, that it is expedient that some parliamentary regulations should be adopted to prevent frauds in the measurement of coals."

Description of several Improvements on Lathes.

Communicated by Mr. J. J. Hawkins, *in a Letter to the Editors.*

With an Engraving.

Gentlemen,

Having experienced much inconvenience in the first establishment of my piano-forte manufactory for want of several little contrivances to expedite the making of certain parts of the instruments, for which no tools in common use would well answer, and having lately attained many useful improvements for those purposes, through the

the ingenuity of some of my friends and workmen, I beg leave to send an account of a few of them for insertion in your valuable Repertory, since they may in all probability prove useful to manufacturers in other branches of business; at least, if not in their present form, they may serve as hints for farther improvements.

My avocations will not at present allow me to describe more than two or three; I shall therefore select such as appear to possess most merit.

My method of tuning piano fortes with screws, each working a sliding piece of brass, to which the string is fastened by means of a hook, required that the hook should be an inclined cylinder, formed out of the sliding piece. This was performed with files, and took considerable time; but by the ingenuity of the workman, M. Chartier, a native of France, who had these pieces to make, I now get the inclined cylinders cut in the turning lathe in a very short time.

To make the tool, a piece of steel is fixed in a chock in the lathe, and a conical hole turned in the end of it; the smallest diameter being outward, and equal to the size of the cylinder wanted. The angle the side of the cone makes with its axis is equal to the required obliquity of the cylinder. The end of this tool is cut like the teeth of a saw, the sharp edges of which are in a plane, perpendicular to its axis.

An inclined plane, parallel to the side of the conical hole, is fitted at the front. A frame containing the article to be cut slides down this inclined plane, when by the revolution of the tool the oblique cylinder is formed, leaving the part surrounding the base quite flat.

A, Fig. 3, (Plate XVII.) is a perspective view of the piece, having the inclined cylinder. B a perspective view of the tool represented as broken off from the lathe *a*,

the end having the conical hole and its face cut into teeth, like a saw or coarse file, either in radii, as at D, or tangents of a circle, not larger than the hole, as at C. E perpendicular section of the whole apparatus. *a* the tool; *b* the oblique cylinder in the piece *c*; *d* a screw, to keep the piece firm in the frame; *e* the frame which slides on the inclined plane *f*.

It is evident on inspection, if the tool *a* be turned, and the frame *e* pushed against it, that an oblique cylinder *b* will be formed. It is scarcely necessary to add, that the frame must be guided down the inclined plane so as to slide quite steady, or the cylinder will be irregular.

My mode of tuning piano fortes also required that I should cut a number of oblong transverse apertures, in a piece of brass about three feet long; to do which I fixed a small circular saw on a long rod in the lathe, but this throwing the saw so far from the treadle, made it necessary to employ two persons. For this inconvenience my father contrived an ingenious remedy: he put the saw on a short mandril, fixed on a puppit, standing obliquely from the axis of the lathe, so that a line produced from this mandril would pass entirely clear of the pulley of the lathe; this small oblique mandril is turned from the mandril of the lathe by an universal joint; the most approved one for the purpose is a hook and eye.

a, Fig. 4, part of the mandril and pulley of the lathe.
b the collar of the lathe.
c the universal joint.
d the small mandril.
e the cutter or saw.
f part of the frame which supports the small mandril.
g the long piece of brass in which are the transverse apertures.

Much

Much time is lost in turning a number of small articles on two centres, with all the common methods of fastening the centre head or sliding puppit of lathes, in consequence of their requiring the workmen to stoop, every time he tightens or loosens it, the fastening being at the bottom.

Numerous advantages, however, arise from having the fastening at the top of the puppit. This method I had from an ingenious Englishman in Philadelphia, but since my arrival in this country, seeing the plan not used by the first-rate lathe-makers, I conclude it is not known here; for I am sure no man would use a lathe fitted in this way, for a few hours, without decidedly giving it the preference; and I think, while on the subject of lathes, that a description will be of use.

Fig. 5, A a plan and B an elevation of the sliding puppit.

a the checks of the lathe.

b a block at the end of the bolt *e*.

d the tightening-nut, with an arm about three or four inches long.

e an iron plate screwed to the top of the puppit, to act as a washer to the nut. This plate must be sunk into the wood till the arm of the nut requires to be turned to the off side, in order to tighten the puppit, because in that situation it is out of the way of the workman.

So soon as I have leisure, I intend describing some more of these little contrivances; which, although they appear trifling, may nevertheless be of considerable importance to manufacturers.

Dalby Terrace, City Road,
Islington, April 17, 1806.

Yours, &c.
JOHN J. HAWKINS.

Experiments on the Oxyd Gas of Azote, made at Thoulouse by a learned Society, and described by M. DISPAN, *Professor of Chemistry in that place.*

From the ANNALES DE CHIMIE.

FOR some years past a variety of experiments have been published upon the effects of the oxyd gas of azote, when introduced into the lungs by respiration. But these experiments, almost always divergent and frequently contradictory in their results, do not, I think, present any thing of a sufficiently positive nature to afford a proper basis for an opinion upon this subject. Such at least was the motive which in the year 12 induced several distinguished cultivators of Chemistry at Thoulouse to associate for the purpose of judging for themselves of the singular properties which the literary Journals attributed to this species of gas. As I can vouch for the purity of the substances that were employed, and in general for the precautions necessary to be taken in such experiments; as I took care at the very time to draw up a faithful account of the sensations which every person experienced; and finally, as these experiments were performed upon a pretty extensive scale, since twelve persons at least subjected themselves to the trial, I conceived them to possess sufficient interest to authorize their publication. I shall merely copy my journal.

Our first meeting was on the 16th Floreal. The nitrate of ammoniac which we employed was confusedly crystallized, but nevertheless perfectly neutral. Its taste was very pungent, and it had a slight smell. It has been formed from its constituent principles by the distillation of sal ammoniac upon potash of commerce, and the simultaneous

taneous saturation of very pure nitric acid by means of the ammoniacal gas produced by this operation.

We first put about a hectogramme of this salt into a small retort placed in the sand-bath. The salt melted, and boiled for some time without yielding gas; at length the retort became filled with a white vapour, which soon disappeared; the gas was then evolved with great rapidity, and we filled several bladders with it. Gradually the evolution subsided; and when we had stopped the operation, there remained hardly any thing in the retort, without however any accident having happened.

Encouraged by this result, we put into a retort of a middling size nearly three hectograms of the same salt, which gave us a quantity of gas sufficient to fill seven or eight bladders with it, although we lost a considerable portion of it. The operation was continued like the first time, and with equal success, till hardly any thing remained in the retort; but a circumstance which surprized us all, and which we knew not to what to attribute, was the formation of an abundant red vapour in the interior of the retort, in proportion as the latter cooled, although the last gas which we had drawn from it contained no nitrous gas at all, as we convinced ourselves by proper experiments.

Effects of the Oxyd Gas of Azote, when introduced into the Lungs by Respiration.

All those who tasted or respired this gas agree that it has a strongly saccharine taste, of which some retained the impression throughout all the remainder of the day. I perceived besides a nitric after-flavour (though indeed that which I tasted was of the last collected). M. de M * *, alluding no doubt to the same sensation, says that he perceived something of a styptic taste. The rest remarked

remarked only the saccharine taste, which indeed is very strongly marked in this gas, especially if we consider the small quantity of matter that is contained in what can be swallowed at a draught.

The following is an exact account of the circumstances which successively took place with all who respired it, which was done in every instance in the same manner, namely, with the aid of a bladder with a stop-cock, the nostrils being closed, and the lungs previously emptied of air as much as possible.

M. G * * suddenly lost his recollection after the third inspiration; he was obliged to be supported for the space of five minutes, after which he raised himself much fatigued, and not recollecting to have experienced any thing else than a sudden swoon and a tingling about the temples.

M. de M * * perceived a saccharine and styptic taste, a great dilatation, accompanied with heat in the chest, swelling of the veins, a hurried pulse. Objects appeared to turn round him. He thinks however that he should have been able to bear a larger dose. The bladder was not sufficiently large for his lungs.

M. de P * * perceived a saccharine taste in the first inspiration, of which he was not sensible in the succeeding ones. His lungs were strongly dilated, with great heat. He felt himself in a very agreeable state after having quitted the bladder, and burst into involuntary and protracted fits of laughter, which it was not in his power to resist.

M. de S. perceived the same saccharine taste as the preceding gentlemen, and retained the impression of it from about ten o'clock in the forenoon till near midnight. He also experienced vertigoes, and his legs titubated

like

like those of a drunken person throughout all the rest of the day.

M. G * * observed the same saccharine taste. On quitting the bladder, he experienced a confusion of the senses, which was succeeded by a very pleasurable sensation which diffused itself over the whole of his body. Titubation of the legs.

M. de C. a saccharine taste, which continued the whole of the day; ringing in the ears; titubation of the legs; a sense of oppression at the stomach. On the whole he considers the sensations he experienced as rather distressing than agreeable.

Being desirous to ascertain to what degree the difficulty of breathing in a bladder might influence the above results, I requested the gentlemen to respire common air in the same manner. They experienced a kind of mechanical fatigue in doing it, but nothing more. In this particular they all coincided.

I wished also to know what effects would be produced by oxygene gas. Those who respired it, with the aid of a bladder, asserted that they found only a slight difference between it and common air, which consisted in increased heat in the lungs.

Hence it must be concluded that the singular effects above described are to be attributed solely to the oxyd gas of azote.

At our second meeting on the 23rd Floreal, our purpose was to repeat the experiments upon the respirability of the oxyd gas of azote on a larger scale.

We put into a retort about eight hectograms of nitrate of ammoniac prepared as before. A connecting tube was adapted to a two-necked flask, out of which the gas was carried by means of a Welter's tube, into the reservoir. The retort was placed upon the sand bath.

Upon

Upon the first impression of the heat the salt was fused; but almost at the same time some reddish vapours, though in small quantity, were formed in the retort. The air of the vessels which the heat disengaged presented also the nitrous smell, so as to cause us to be apprehensive for the success of the operation; but these vapours and this smell insensibly diminished, and at last entirely disappeared. About this time the bubbles that formed, exhaled a very perceptible smell of prussic acid, which even continued for a considerable length of time. At last the retort became filled with white vapours, and the oxyd gas of azote began to pass over. The disengagement soon became so abundant that it was thought necessary to remove the fire; but on applying it anew, the gas, the evolution of which had abated in the interval, appeared again in such abundance, that in one place the luting gave way. Notwithstanding the considerable loss which took place through this aperture, the evolution continued with extreme rapidity in the reservoir for at least a quarter of an hour.

This circumstance gave us reason to suppose that, if the lute had not given way, an explosion must infallibly have taken place, and that it may perhaps be unnecessary to look for any other cause of the accidents which are said to have sometimes occurred in this operation. So much at least I can affirm, that this was the third time I had repeated it, without any thing of the kind having occurred to me; and yet I have always urged the fire till nothing remained in the retort, and I have operated successively upon 1, 2, and this last time upon 8 hectogrammes of salt.

Let us now proceed to what respects the respiration of the gas. Twelve persons were subjected to the experiment

ment, and several of them twice. It must be observe that several of them had already respired the gas at our former meeting, when two out of seven had experienced pleasurable sensations; but this time there was no reason for jealousy. None experienced any pleasing sensations, not even the two just mentioned. On the contrary some suffered, and that in a very acute degree.

M. de M. stamped with his foot during the whole time that he held the bladder; then recovering from a profound stupefaction, he said he had felt a sensation at the back of his head like the stroke of a dagger, sufficient to knock down an ox, and that he would not experience the same sensation for all the world. The others were in general affected with dizziness, glare before the eyes, which symtoms were succeeded with some by fits of laughter. I in particular was of this number; and the following is an exact account of the sensations I experienced.

By the first inspiration I emptied the bladder. A saccharine taste immediately pervaded my mouth, and my whole chest was dilated with pleasure. I emptied my lungs and filled them again; but, the third time, I felt a ringing in my ears, and I quitted the bladder. Then, without entirely losing my recollection, I remained for a moment rolling my eyes about in a kind of stupefaction; after which, without being aware of it, I burst into such fits of laughing as I had never experienced in my life. After some seconds this propensity to laughter suddenly ceased, and I experienced no further symptoms.

M. de P * * felt at first no other effect than convulsive motions in some of the muscles of his face. But in the course of the day he was affected with a violent diarrhœa and the same thing happened also with M. D * *.

Upon the whole it would be very difficult to give an accurate general account of the effects of the acid gas of azote, as they vary so considerably in different individuals, and, what is more remarkable, in the same person. M. de S * * who respired it four times, says that he experienced each time sensations which he had never felt before. For my part, having repeated the experiment at the same meeting, I did not feel again the same propensity to laughter with which I had before been affected. I should only have fallen into a swoon, had I continued the experiment longer.

Effects of the Oxyd Gas of Azote upon Brute Animals.

On this subject I have only a single experiment to relate, but its result appears to me sufficiently interesting to render it worthy of being recorded.

I put a greenfinch into a pretty wide bottle full of oxyd gas of azote. The bird did not at first appear at all to suffer, but it soon closed its eyes and laid itself gently upon its side, as if fallen asleep. When taken out into the open air, it rose upon its legs, without attempting to fly away. Subjected a second time to the same experiment an hour after, and kept under it for a greater length of time, it was taken out motionless, and no efforts were capable of restoring it to animation.

In my opinion, it is very remarkable that this bird made no efforts to escape, and experienced no convulsions, as happens in other gases.

Considerations

Considerations on the Oxydation of Metals in general, and on that of Iron in particular. By M. THENARD.

(Concluded from Page 399.)

THE acidulous and acid sulphates of green iron, both of which result from the combination of the green oxyd of iron with sulphuric acid, exhibit a still greater difference than those which we have just examined. The first (that which has but little acidity) does not crystallize; being able to exist only in the liquid state, if we subject it to evaporation, it absorbs oxygene from the air, becomes turbid and deposits neutral, yellow, insoluble and highly oxydated sulphate; it is thus converted into acid sulphate, in which the oxyd is always green, which has hardly any colour, and which opposes much greater resistance to every kind of decomposition. Though it has green oxyd for its base, it is of a red colour, and it is this circumstance which has deceived most chemists, who have hitherto considered it as a highly oxygenated sulphate. Hence also it happens that the acidulous sulphate of white iron, the solution of which is of a beautiful green colour, becomes red by exposure to the air.

Oxygenated muriatic acid converts it immediately into highly oxydated acid sulphate; iron, on the contrary, into green or little oxydated acidulous sulphate; sulphuric acid causes it immediately to lose its red colour, and converts it into acid sulphate, which is almost colourless, or which at most has a slight green tinge. This acid sulphate crystallizes by a well-conducted evaporation; the crystals which it forms have something of an emerald green colour, and in this respect it approximates to the little oxydated acid sulphate. They neither effloresce nor deliquesce; their solution in water does not, like the acid

sulphate of white iron, absorb oxygene, unless after a great length of time: the excess of acid which they contain fixes the oxyd of iron to a certain degree; nevertheless by the action of oxygenated muriatic acid the oxyd becomes red, and by that of iron it is converted into the state of white oxyd, especially at the temperature of boiling water; the acidulous and acid sulphates of ...., iron are both precipitated in a green state by the alkalies. The precipitate contains no acid when a great excess of base is added, and especially when heat is applied; in every case it is always green. It is by treating the red oxyd of iron with dilute sulphuric acid, that the acid sulphate of red iron is obtained; the solution would not take place if the acid were concentrated; the acid sulphate of red iron contains more acid in excess than the acid sulphate of green iron, and this more than the acid sulphate that is little oxydated. These properties it possesses in common with all the oxyds of one and the same genus; they require the less of any acid for their saturation, as they contain less oxygene; and on the other hand they require the more, the more they are oxygenated; as is proved in the cases of antimony, mercury, tin, copper, and iron. They alone, among all the metals, combine at different degrees of oxygenation with the acids; and they are all, without exception, subject to this law. This is the reason why the solution of little oxydated muriate of tin becomes turbid in course of time by the contact of the air, or when, being crystallized, we attempt to dissolve it in aerated water; it is also for the same reason that the same solution of tin, recently prepared, entirely decomposes corrosive sublimate, and revives the mercury; it absorbs from it not only the oxygene, but also the muriatic acid; hence it likewise proceeds that the acid sulphate of white iron is partly transformed, by absorbing the oxygene of the atmosphere, into acidulous

sulphate

sulphate of green iron, and assumes a slight red tinge; hence finally the same salt, when it is only acidulous, is immediately rendered turbid by pouring aerated water into its solution. Several other phenomena also depend upon the same cause; but were I to dwell longer upon them, it would carry me too far from the subject which I propose to treat.

Like the two other acid sulphates, the acid sulphate of red iron is almost colourless; it acquires a very high red colour when its excess of acid is in part saturated by potash; by the addition of a larger quantity of salifiable base we precipitate from it a neutral sulphate, which nevertheless is capable of being decomposed by the alkalies; like all the other highly oxygenated salts of iron, it separates sulphur from hydrogene, and re-passes into the state of very acid sulphate of green or white iron; it gives no crystals by evaporation; iron converts it into less oxygenated sulphate.

I have very little to say respecting the highly oxydated neutral sulphate. It is yellow, insoluble; it is that which is precipitated in the course of time from the solutions of acidulous sulphate of green and white iron, when exposed to the air; it is also that which is deposited when we evaporate the solution of the acidulous sulphate of green iron, which from being red becomes almost colourless, because it then passes into the state of acid sulphate: finally, this is the salt which chemists formerly conceived to be a particular oxyd, and which led them to admit a yellow oxyd of iron, intermediary between the red and the green.

The action of the muriatic and the nitric acids upon iron, greatly resembles that of the sulphuric acid; that of the muriatic acid does not differ from it, but the acid muriates of iron are by no means so well characterized as the acid sulphates;

phates; the little oxydated muriate crystallizes very well, and the crystals which it forms are of a beautiful green colour; but when the solution is exposed to the air, the iron becomes more oxydated; and as it then requires a larger quantity of muriatic acid for its solution, a part of it is precipitated; however, there exists no nitrate in which the oxyd is white. None but nitrate of green iron and nitrate of red iron can be obtained; and the first is formed only by taking acid at 5°, and the second only by employing acid at 12° to 15° at most. If this acid were more concentrated, a portion of the red oxyd would be precipitated, and but very little would be found in solution if it had 36° to 40°. This precipitation undoubtedly takes place only because the red oxyd is less divided; to this cause also we must attribute the circumstance that nitric acid has no action upon colcothar, and that the sulphuric acid itself has so little action upon this substance. What proves this is the property which these two acids possess of dissolving easily the red oxyd in the gelatinous state, or recently precipitated from the nitrate or muriate by the alkalies.

We find also in the other acids the same mode of action as in the sulphuric acid; but most of the salts which result from them being insoluble, are obtained rather by the way of double decompositions than in a direct manner. I shall not here consider all these saline bodies, on account of the little importance of which they are at present to science and the arts: I shall only examine the two most important of them, namely, the gallates which form the bases of black dyes, and the prussiates, the uses of which have been so much extended for thirty years past.

We know that the gallic acid attacks iron even at the temperature of the atmosphere; that it dissolves it with a disen-

disengagement of hydrogene gas proceeding from water decomposed; that this solution, which at first is colourless, soon becomes blue by exposure to the air, and that then it speedily becomes turbid, and passes into a blackish grey. All these phenomena have been described with great accuracy by Professor Proust; though as yet they have not been satisfactorily explained. I was obliged to attribute the cause of their difference to the three oxyds which iron is capable of forming. Thus when gallic acid is poured into acidulous sulphate of white iron, no precipitate is obtained. With the acidulous and acid sulphate of green iron, one of a beautiful blue colour is formed; and if we repeat the experiment with a highly oxygenated salt of iron, the matter that is deposited is of a black colour, inclining to a grey. Nevertheless an excess of acid may prevent the deposit from taking place. This is what is remarked in the acidulous sulphate of white iron, where the addition of an alkali is indispensable for the formation of the gallate, which then presents itself in the form of violet-coloured flakes; this is what is also observed in the acid sulphate of red iron, where the saturation of the acid is necessary for the precipitation of the compound; and what, on the contrary, does not take place in the muriate of red iron that is but little acid.

The three oxyds of iron of which I have been speaking, and which can all combine with the acids, form with the prussic acid still much more multiplied combinations than those which we have been examining. There not only exist prussiates of iron both neutral and with excess of oxyd, but they both are capable of uniting with the prussiate of potash, and forming insoluble triple salts, if the metallic prussiate predominates, and insoluble ones, on the contrary, when there is hardly any thing more than alkaline prussiate. Such is, in a few words, the general history

history of Prussian blue; but it is of too great importance to science for us to neglect the particular consideration of each of the parts which compose it.

One of the points of this history most useful to be studied, is the different coloration of the precipitates that are obtained by decomposing the solutions of iron by the alkaline prussiates, for their colours vary in a very singular manner. Sometimes they are white, sometimes greenish, sometimes more or less blue, and between these different tints, which are the principal, an eye accustomed to judge of colours can distinguish many others. These effects do not depend solely upon the state of oxydation of the iron; they depend also upon the state of the alkaline prussiate, and of the metallic solution.

However little oxydated the iron may be, the solution little acid, and the prussiate with excess of alkali, a white precipitate will be obtained; it will be of a greenish-white if, the other circumstances remaining the same, the prussiate is neutral; the one evidently differs from the other only by the proportion of the principles which constitute it; the first contains an excess of oxyd, while in the second the acid is merely saturated with it; accordingly the acids immediately render the prussiate of white iron greenish, by taking from it a portion of its base and the alkalies cause the slightly green prussiate of iron to assume the white colour, by seizing a part of the acid which enters into its composition. Art therefore can transform at pleasure these two salts into one another, by causing the quantities of the substances which form them to vary.

These precipitates, however, are not, as has long been imagined, mere prussiates of iron; they contain also prussiate of potash, as has been proved by M. Berthollet. The prussiate of potash has even so great an affinity for the

the prussiate of iron, that the sulphuric acid decomposes only in part the insoluble combination which these two salts form together. When we add only a slight excess of acid, the residuum still contains a large portion of potash, and is sensibly green; it becomes of a greenish-blue by the addition of a larger quantity of acid; and if we analize it, we find in it less of alkali and oxyd. This attraction is still more striking when the prussiate of potash is in sufficient abundance to render the combination soluble; then the sulphuric acid does not evolve from it even the slightest odour of bitter almonds, and the sulphate of iron always produces a very considerable precipitate in it; whereas, when this alkaline salt is pure, the carbonic acid is sufficiently powerful to disengage the prussic acid from it.

The same results are obtained by substituting in place of the little oxydated acidulous sulphate of iron the acidulous sulphate of green iron and the muriate of red iron, which has but little acidity; its precipitates are always triple prussiates of iron and potash; that of which the oxyd is green is less blue than that of which the oxyd is red; both change their colour when they are united with a great excess of oxyd; the first becomes of a yellowish-white, and the second of a reddish-yellow; if the excess of oxyd is less considerable, they approach more to the blue colour; but both are susceptible of being freshened by the acids.

There exist, therefore, six well-characterized prussiates of iron; three neutral, and three with excess of oxyd: the excess of oxyd may be more or less considerable, which occasions a singular variation in the colour of these prussiates. These six prussiates of iron may combine with different quantities of prussiate of potash, and undoubtedly also with the other alkaline prussiates, so

that this genus of salts is very numerous in species. We observe in general, that those which contain much alkaline prussiate are soluble; those, on the contrary, which contain much prussiate of iron are insoluble. It is known that Prussian blue becomes green by exposure to the air, and that there is then formed oxygenated prussiate of red iron. When ordinary prussiate of potash is boiled with Prussian blue, which is thus oxygenated either wholly or in part, the prussic acid seizes upon the potash, and the prussic acid, which was combined with the latter, upon the oxyd of iron, so that the filtrated liquor gives a green precipitate with the nitrate or muriate of red iron. This is the reason why certain solutions of precipitates, made by means of Prussian blue, give a white precipitate with the little oxydated sulphate of iron instead of a blue one.

It is this variety which the prussiates exhibit in their composition, that renders their preparation so difficult. This however is not the only obstacle attending the fabrication of Prussian blue: there are several which it has required a long time to surmount, and some of them subsist even at the present day. One of the most necessary improvements to be made in this art would be to apply to some useful purpose the large quantity of carbonate of ammoniac which is formed by the calcination, the employment of which would render it possible to introduce the prussiate of iron with much greater advantage into commerce. I have ascertained by experiments made with great accuracy, that when the potash is calcined with animal matters, as much carbonate of ammoniac and prussiate of potash is obtained as when they are distilled alone; likewise that quite as much prussiate of iron is formed with the coal that results from their distillation, as with all their principles combined. Sal ammoniac may

therefore

therefore be made at the same time with Prussian blue, and the products consequently doubled, without any augmentation of expense in the fabrication.

The manufacturers of Prussian blue ought to direct their attention not only to this circumstance, but also to several others whereby their processes may be improved. Some of them do not employ the most advantageous proportions of potash and of blood; none of them add iron; several apply the heat for too great a length of time; but few cause the prussiate of potash to crystallize; most of them, not knowing that, in the calcination of the matters, the sulphate of potash contained in the potash which they use, is converted into sulphurets, are of course ignorant of the means of preventing their bad effects; finally, they almost all bestow too much time upon oxygenating the Prussian blue, whereby they augment the labour, and deprive themselves of the opportunity of preparing much more considerable quantities. Nevertheless the prosperity of their manufactories depends upon all these considerations united. The following is what I have remarked upon this subject. Experiment has proved to me that a quantity of potash equal to that of blood is preferable to every other proportion. It is known that the addition of iron favours the formation of the prussiate of potash, and fixes it. It is no less evident that the due management of the fire is of equal importance, since by exceeding the period of the fusion, we decompose in part the prussiate of potash or the matter which ought to form it. It is equally necessary to make the prussiate of potash crystallize, especially if it be intended to obtain Prussian blue of superior quality. On the one hand we thus convert the sulphurated hydro-sulphuret of potash into sulphate of potash, and on the other it is then necessary to add but little alum to saturate the excess of alkali. We might also decompose the sulphurated hydro-sulphuret of potash

by the sulphuric acid, and the more so as this acid does not alter the prussiate of potash combined with a certain quantity of prussiate of iron. This crystallization is not indispensably necessary in the fabrication of ordinary Prussian blue, into which there enters much alumine, which cannot be separated except by a large quantity of potash; it is sufficient to let the liquor boil for some time, in order to burn the hydro-sulphuret, otherwise the sulphurated sulphuret is separated by the aid of the alum. Finally, the operation would be greatly abridged, if, instead of washing the prussiate of iron in a large quantity of water, it were mixed with a small quantity of oxygenated muriate of lime.

Such are the various observations which I thought it necessary to unite in this memoir, though I cannot claim them all as my own. In support of those which are peculiar to myself, I have quoted some which are the fruit of the labours of many different chemists. Sometimes even I have recorded facts which have long been known, in order to exhibit the aggregate of the properties of a body, or the method which ought to be followed in an operation. Nevertheless I flatter myself I have examined, in a manner interesting to science and the arts, the oxydation of iron and the combination of its oxyds with the acids. The existence of the white oxyd of iron enables us to comprehend several phenomena, the cause of which was hitherto unknown; in particular, it explains that variety of colours which we observe in all the ferruginous salts. It throws some light upon one of the most important and beautiful of dyes, namely the black dye. It no less illustrates the fabrication of Prussian blue, with regard to which so much remains still to be done. Finally, it is of great importance to the art of obtaining the sulphate of iron, the consumption of which becomes from day to day more considerable.

Experiments

Experiments on a Liquor which was found contained in the Caoutchouc of the Castilloya Elastica of Mexico, brought over by Messrs. HUMBOLDT *and* BOMPLAND.

By Messrs. FOURCROY *and* VAUQUELIN.

From the ANNALES DE CHIMIE.

AFTER breaking the bottle which contained the concrete elastic gum, the latter was put into a plate. It was of a greenish-white colour.

Gradually the particles of this elastic gum approached together, as if by a central attraction, and there issued from them, in the form of drops, an abundance of a brownish-red colour, which had a pungent and bitter taste, and which reddened turnsole paper.

When the contraction of the matter seemed to have ceased, and no more liquor exuded, it was put into water, in which it remained suspended, by reason of its superior lightness, in order that it might be drained of all its juice, and obtained in greater purity.

Some drops of this liquor, which had accidentally fallen upon iron, having quickly assumed a blackish colour, it was suspected to contain tannin or gallic acid; but it did not throw down a black precipitate from the solutions of iron, as these substances do; on the contrary, it threw down one of a deep green.

This liquor, subjected to some experiments with the re-agents, presented the following phenomena:

1. It reddens the tincture of turnsole.
2. It does not coagulate glue.
3. It precipitates the sulphate of iron in a green state.
4. It precipitates the acetate of lead, and the nitrates of mercury and silver, in a yellowish-white.
5. Alcohol coagulates it in brown flakes.
6. Evaporated to the consistence of a syrup, and heated with boiling alcohol, it gives it a red colour: on cooling,

cooling, the alcohol deposits a pulverulent white matter, which has the following properties:

a, it has no taste; *b*, it is soluble in alcohol, when humid; *c*, it is almost insoluble in water; *d*, it reddens the tincture of turnsole; *e*, it burns like the animal matter, that is to say, swelling and exhaling a fetid and ammoniacal odour; *f*, it leaves hardly any ashes after combustion; *g*, it combines easily with the alkalies, from which it is afterwards precipitated by the acids; *h*, its alkaline solution precipitates the solutions of iron in a green state.

By evaporation of the alcohol, a fresh quantity of matter, similar to that which is deposited by the cooling, is precipitated in the form of yellowish white flakes; but there remains in the alcohol another matter which gives it a yellowish-red colour, considerable bitterness, and which does not separate from it. The properties presented by this substance are as follows:

A. An extremely bitter taste, analogous to that of aloes or of colocynth.

B. It is soluble in water; its solution gives to the sulphate of iron an emerald green colour, without forming a precipitate in it.

C. It powerfully reddens turnsole paper.

D. It produces a brisk effervescence with sulphuric acid, attended with a disengagement of a pungent vapour similar to that of concentrated sulphuric acid: the mixture of these substances yields a precipitate with water.

E. The aqueous solution of this matter coagulates with the nitric, muriatic, &c. acids, in the form of yellowish flakes.

F. It swells up on ignited coals, diffusing a white pungent vapour like that of vegetable matters.

G. Once separated from the alcohol, and reduced to the consistence of a thick extract, it dissolves in it again only

only in small quantity; however, it still communicates to it a sligh colour, and the property of turning the solutions of . on green.

H. A certain quantity of this substance, in the form of extract, was subjected to the action of heat in a crucible of platina. It swelled up in a singular manner; it exhaled at first a pungent smell of acetic acid, and towards the end an empyreumatic and slightly-ammoniacal odour. Its coal, in a state of extreme division, burnt with facility, and in proportion as the combustion advanced, the substance became soft, and seemed to melt. As these phenomena indicated the presence of an alkaline substance, the combustion was stopped in order to prevent the volatilization of alkali, and the matter was lixiviated and filtrated. The liquor which was clear and colourless, being subjected to evaporation, yielded a gramme and eighty-five hundredths of slightly-carbonated potash, mixed with muriate of potash. As to the elastic gum itself, it presented no difference from ordinary elastic gum; it was very white, very elastic, lighter than water, acquired a slight brown tinge in the air, in a less degree however after having been well washed than the common elastic gums; which seems to indicate that the greater part of the colour of these substances proceeds from some remains of brown juice which collects at the surface in proportion as the substance dries.

The part of the juice of the elastic gum which is insoluble in alcohol, and which, as has been mentioned above, was coagulated by this liquor, dissolves easily and completely in water: the solution is not precipitated by the infusion of galls, which seems to indicate that it is not of an animal nature, and yet it furnishes by distillation a large quantity of crystallized carbonate of ammoniac, and thick red and fetid oil like that of the animal substances; its coal left, after burning, a large quantity of grey ashes, which

which consisted of oxydated iron, silex, and a small quantity of sulphate of lime.

It follows from these experiments, that the juice in which the elastic gum is formed consists of at least four matters. The *first* is brown, insoluble in alcohol, both hot and cold, highly soluble in water; it yields much oil and carbonate of ammoniac in its decomposition by fire; nevertheless it is not precipitated from its aqueous solution by tannin, as are in general all the highly-azoted substances, which form a precipitate of a dingy green colour with the solutions of iron.

The *second* is white, pulverulent, without taste or smell, soluble in boiling alcohol, from which it is separated by cooling; not soluble in water, but soluble in the caustic alkalies, from which the acids precipitate it in the form of flakes; it reddens the tincture of turnsole, and finally it yields much ammoniac and oil in distillation. This is an acid matter.

The *third* is yellow, and has a bitter taste, similar to that of aloes; it is soluble in alcohol and in water, but more abundantly in the latter; it gives a green colour to the solutions of iron, without forming a precipitate in them; it also furnishes a small quantity of ammoniac by its decomposition.

The *fourth*, finally, is acetate of potash with excess of acid, and mixed with a small quantity of muriate of potash. It is this acetate which, by its decomposition, gave the quantity of potash of which we have spoken above.

It is possible that the free acid when the juice of the the elastic gnm contains, may have beon formed by fermentation after the extraction of ihe juice; it is even possible that that which is combined with the potash may have been equally so, and that the potash may before have been combined with the elastic gum, which then separated

separated from the liquor in proportion as this fermentation took place; nothing however can ascertain this point but the examination of the fresh juice. Should the cause actually be such as has here been suggested, it would be easy to conceive the solution of the elastic gum in the juice of the tree, and its precipitation by the acetous fermentation: however, this explanation cannot apply to all the juices which contain elastic gum, for there are some of them which contain no alkalies.

Experiments on the Eudiometrical Means, and on the Proportion of the Constituent Principles of the Atmosphere.

By Messrs. HUMBOLDT *and* GAY-LUSSAC.

(Concluded from Page 381.)

Of the Nature of the Air extracted from Water, and of the Action of Water upon the Gases, both pure and mixed.

WE have hitherto examined the eudiometrical means which lead to the exact analysis of the atmospheric air. We should undoubtedly have confined ourselves to the enunciation of the principal facts to which the first part of our investigation has conducted us, had we not remarked in the course of these experiments, and particularly of those upon the sulphurets, that water and other liquids exert an action upon the air which may frequently become a cause of error; the more important as it has hitherto been little attended to. We therefore apprehended, that we should leave our labours in a still more imperfect state than we have already done, if we had not directed our inquiries to this action of water upon the gases, both pure and mixed, which are subjected to it. The experiments which we have made with a view to this object, shall form the conclusion of our memoir.

It is generally known that water can hold air in solution. Boyle, Huggens, and Mairan, have discussed this fact; but they did not possess the means of discovering that this dissolved air differs chemically from the atmospheric air. The celebrated Priestley was the first who observed that the air extracted from water contains more oxygene than common air. M. Hassenfratz afterwards asserted that rain-water evolved an air which contained nearly forty-hundredths of oxygene; and Messrs. Ingenhoutz and Breda, in their experiments upon nitrous gas, were led to similar results.

But if it is already known that the air contained in water is more pure than atmospheric air, it has also been maintained that water absorbs the oxygene gas more abundantly and more readily than the azote. M. Fourcroy even mentions the curious fact, which however he himself does not conceive to be sufficiently authenticated, that water charged with oxygene gas absorbs hydrogene gas, upon which common water has hardly any action. We shall see hereafter, that that which it exercises upon any gas is modified by the nature of the air which it already holds in solution.

Mr. Henry, in a memoir lately published in England, has examined the absorption of different gases by water deprived of air. He effected these absorptions under a pressure equal to that of two or three atmospheres; but he has not treated of the mixture of different gases, nor of the affinity which water has for this mixture: he confines himself to the examination of the quantity absorbed, according to the difference of temperature and of barometric pressure, without directing his inquiries to the action of water already saturated with other gases.

We conceived that we ought not to neglect a subject so intimately connected with eudiometrical inquiries, and to which chemists have hitherto paid so little attention.

We

We have examined the degree of affinity by which the oxygene dissolved in water is retained in it, according to the temperature and the salts which it may contain. We placed in contact with water equal quantities of gases, both pure and mixed, and we observed the changes which these mixtures undergo in their chemical composition. Finally, we have begun to examine a problem of great importance to meteorology, namely, whether rain water holds hydrogene in solution.

All these inquiries, which we intend to prosecute during the course of this year, and particularly upon the mountains which we purpose to visit, are not as yet in a very advanced state; we shall therefore content ourselves for the present with offering some leading facts, which, we trust, will not be deemed altogether destitute of interest by philosophers.

After mixing the whole mass of air which water yields by boiling, without separating the portions first disengaged from those evolved at the end of the operation, we found by Volta's eudiometer that distilled water, which has re-absorbed atmospheric air, gives an air which contains in the hundred parts 32,8 of oxygene; the water of the Seine, one which contains 31,9 oxygene; and rain-water an air containing 31,0 of the same principle.

From these experiments it follows, that we may extract from these three waters air nearly equally rich in oxygene, and purer by ten-hundredths than the atmospheric air. This quantity of oxygene is more variable in the waters of wells, which in the bowels of the earth are in contact with substances that exercise an affinity upon oxygene. Water of the Seine, collected at another period, yielded an air containing only 29,1 of oxygene, an air somewhat less pure than that of the rain water.

If distilled water which has again absorbed air, rain water, and river water, yield airs all of which are much

purer than the atmospheric air, it will be still more interesting to examine the nature of the gaseous mixtures which water yields when heated gradually. It is in these experiments that the great affinity of oxygene for this liquid displays itself in its strongest light. We gradually heated water of the Seine to ebullition, and collected the air which is disengaged by successive and unequal portions. We took 200 parts of these portions, and having detonated them with 200 parts of hydrogene gas, they gave us the following results.

Portions of air, according to the order of their evolution.	Absorption.	Oxygene gas contained in 100 parts of the air evolved.
First	142,0	23,7
Second	164,5	27,4
Third	185,0	30,2
Fourth	195,0	32,5

These experiments, several times repeated, prove that water abandons at first an air, the purity of which is only a little superior to that of atmospheric air; after which the purity of this air, or the disengagement of oxygene, progressively increases, and the last gaseous portions which the heat separates contain the most oxygene. On repeating this experiment upon snow-water, the first portions of air had 24,0; the last 34,8 of oxygene. Possibly if the mass of water were heated still more slowly, and the small portion of air which is first evolved carefully separated, we should have at the beginning of the operation an air still less pure than that which we obtained.

Water therefore does not exercise an uniform action upon oxygene and upon azote, and elevation of temperature diminishes its action upon the first less than upon the last. It is even probable that the portion of air which is disengaged towards the end of the operation would be of greater purity than that of 32 or 34 *per cent.* of oxygene,

gene, if the water contained in the vessel which receives the gaseous mixture did not begin to be heated, and to evolve its air, which then is only at 23 *per cent.* of oxygene. This disengagement takes place especially when the aqueous vapour begins to pass, and it is this diminution of the purity of the air expelled last and the inequality of volume of the four separate portions, which explain how the whole mass of air extracted at once contains to the amount of 31 hundredths of oxygene.

This unequal action of water upon the oxygene and the azote manifests itself also in the solution of salts. We have observed that the pure water of the Seine gave by ebullition nearly one half more of air than the same water charged with muriate of soda. The cause of this diminution consists in the very considerable quantity of air which already disengaged from the water, still cold, whilst the solution of the salt is taking place. This air, accurately analyzed, shewed only 0,225 of oxygene, whilst the air obtained by boiling the water charged with muriate of soda, contained 0,305. Hence it appears that the water, in dissolving the salt, abandons a part of the air which it holds in solution, but that this part contains oxygene in a less proportion than that which it retains.

The condensation which water experiences in passing from the liquid to the solid state presents to us a third class of phenomena analogous to those which we have just described. Melted ice yields only about half the quantity of air which we obtain from common water, and it is to be observed that it does not begin to evolve its air until its temperature has been raised as high as above the sixtieth degree of the centigrade thermometer. The air obtained, divided into two unequal portions, shewed in Volta's eudiometer 27,5 and 33,5 of oxygene. Then the purest air was again evolved the last.

The

The small quantity and the great purity of the air evolved from melted ice, proves that water, as it passes into the solid state, abandons a large portion of its air, and that this part, separated during the congelation, is air of much less purity than that which it retains. Thus three phenomena, which at the first view appear different, water at a temperature from 35° to 40° centigram, water dissolving salts cold, and pure water congealing into ice, present results entirely similar to each other in their action upon oxygene and azote. A moderate temperature acts like the solution of a salt, and this like the transition from the liquid into the solid state. The water in these three cases evolves an air of less purity than that which it holds in solution.

It is a very remarkable phenomenon that the congelation of water into the state of snow expels less air from it than the formation of ice. We melted some newly fallen snow, and heating it gradually, we obtained a volume of air nearly twice as great as that which melted ice yields. The air extracted from the snow-water was almost in equal abundance with that evolved from the water of the Seine; for the latter gave by ebullition 1940 measures of air, while the same volume of snow-water yielded 1892. These 1892 parts collected into 5 portions, according to the periods at which they were expelled by the heat, shewed successively in Volta's eudiometer.

The first	24,0 of oxygene.
Second	26,8.
Third	29,6.
Fourth	32,0.
Fifth	34,8.

This last portion of air is the purest we have yet extracted from any water.

The

The volumes of each portion being known, calculation gives us for the purity of the air considered collectively 28,7 of oxygene. The water of the Seine yielded on the same day an air which was less pure by $\frac{4}{1000}$. Otherwise the two waters, that of melted snow and that of the river, give a volume of air equal to nearly $\frac{1}{25}$ of their own.

These experiments upon snow-water and upon melted ice, which we design hereafter to vary in many ways, present considerations of great importance to the study of meteorology. Snow is nothing else than an aggregate of small crystals of ice which are formed in the higher regions of the atmosphere, and yet these small crystals melted give a volume of air nearly twice as great as that which the ice formed upon our rivers yields. Hence it is to be concluded that when the water dissolved in the air congeals into snow, it does not expel that large portion of air which it disengages in its congelation upon the surface of the earth, unless we might be allowed to suppose that snow retains between its small crystals a certain quantity of air which it absorbs as it melts; for it appears that it is principally at the moment of its congelation that water abandons the greater part of its air.

The beautiful vegetation which surrounds the Glaciers, the rapid developement of plants when the snow melts in the spring, and several phenomena which are supposed to have been observed in agriculture and in bleaching, have led to the notion that the waters of ice, snow and rain, produced peculiar effects by a large quantity of dissolved oxygene which was evolved from them. The experiments which we have hitherto made do not seem favourable to these conjectures. There undoubtedly exist wells, the waters of which contain air inferior in purity to the atmospheric air, and we do not doubt that these waters, charged besides with salts and carbonic

bonic acid, must have an influence upon vegetation and bleaching very different from that of snow-water. But the differences produced by distilled water exposed to the air, rain-water, snow-water and the water of the Seine, are not easily to be accounted for by the oxygene dissolved in them, if we recollect that all these airs contain water of nearly equal purity, and that they contain it in almost equal quantity. The phenomena of vegetation, like those of meteorology, are so complicated; they depend upon the concurrence of so many causes at once, that we must be very careful not to attribute to one what is the effect of many.

The experiments which we have recorded on the force with which the last particles of oxygene dissolved are retained by water, throw an additional light upon the state in which air exists in liquids. The specific gravity of distilled water and of that charged with air, being apparently the same, Mairan has concluded with reason that this air cannot be lodged in the fluids in an elastic state. The chemical phenomena confirm this opinion. If water deprived of its air by distillation, or by the air-pump, could be considered as a sponge the pores of which are empty, how should these pores not fill themselves on the first contact with atmospheric air? But this solution of air in water can be considered only as the effect of a chemical affinity; for how but by this affinity could the absorption of the gases by the water deprived of air be so slow, and above all, how else should the water dissolve one gas in preference to another? How should water charged with one species of air abandon a part of it, as we shall see hereafter that it does, to receive another of a different nature?

After having examined the air which may be extracted from water under different circumstances, we shall conclude our Memoir with the experiments which we have made

made in placing gases, both pure and mixed, in contact with water. The gases which we employed were exactly of the same volume, and the quantity of filtrated water of the Seine was nearly equal. After a space of from 6 to 8 days, we not only measured the quantity of the volumes absorbed, but we also analyzed the residues. This analysis was the more necessary as one might often be tempted to conclude from a very small change in the volume of the gas placed in contact with the water, that the latter has no sensible action upon it, whilst the nature of the residue indicates that this action has been very strong, but disguised by the quantity of air evolved from the water in lieu of that which has been absorbed.

Of all the gases, the oxygene is that which the water of the Seine absorbs in the most considerable quantity. If we place in contact with this water, already charged with air, 100 parts of oxygene gas, 100 of azote, and 100 of hydrogene, the oxygene gas will have diminished by 40 parts when the two others have lost only 5 and 3 parts. But the real absorption of the oxygene gas is still much more considerable than its apparent diminution indicates. The 60 parts of residue, instead of being pure oxygene, contained 37 parts of azote and only 24 of oxygene; so that the 100 parts of oxygene employed, had lost upon the Seine water 77 parts, which had expelled 37 parts of azote. Thus a river water which had been exposed for a long time to the atmosphere, and which might be considered as saturated with air, absorbs a large quantity of oxygene when it is presented to it. It takes it, without abandoning a portion of azote equal in volume to the oxygene absorbed.

The action of water upon the volume of hydrogene gas appears to amount almost to nothing. The inequa-

lity of the results which we obtained prevent our saying any thing concerning the slight changes which it may undergo during their contact.

The volume of pure azote gas diminishes upon the water by between 2 and 3 hundredth parts; but the residue is no longer pure azote: we discovered in it 11 parts of oxygene which had been displaced from the water by 14 parts of azote. The azote therefore dislodges ozygene from the water, as oxygene dislodges azote. The action is analogous, but the quantities absorbed and dislodged are different.

The contact of the river-water with a mixture of hydrogene and oxygene gases was examined under various circumstances. Sometimes we mixed the two gases in equal quantities, sometimes we caused one or the other to predominate. The diminution of the volume of the gases is greater when the oxygene predominates; that is to say, when we expose to the water a mixture of 200 parts of oxygene with 100 of hydrogene. In all these experiments azote is again dislodged from the water. In analyzing the residuum of a mixture of equal parts of oxygene and of hydrogene, we found in 100 parts 20 parts azote, 50 hydrogene, and 30 oxygene. The greater the absorption of oxygene was, the more azote we found to be dislodged. Having mixed 400 parts of oxygene with 200 of hydrogene, this volume was reduced upon Seine water, in the space of ten days, from 600 parts to 562. If this residue had undergone no chemical change in its proportions, if no other gas had been dislodged, it ought to have contained 375 parts of oxygene and 187 of hydrogene; but our analysis shewed it to contain 246 azote, 142 hydrogene, and 174 oxygene.

These experiments prove that hydrogene, which, when placed alone in contact with water, is not sensibly absorbed

sorbed by it, is dissolved in it, and that in a very considerable proportion, when it is mixed with oxygene. On this subject a question of great importance to Natural Philosophy presents itself; namely, whether this hydrogene absorbed by the water exists in it in the state of hydrogene, or whether it combines in it with oxygene and so forms water. We have endeavoured to solve this question by leaving a mixture of hydrogene and oxygene in contact with water recently deprived of all its air by ebullition. After the space of 12 days, we distilled this water, and on analyzing the air disengaged from it, we found the hydrogene to exist in it in such abundance that we could inflame it in Volta's eudiometer without adding to it any other gas. This experiment proves beyond a doubt that the hydrogene absorbed is found again in the water. But did this water give up again the same quantity which it had absorbed? Would not this hydrogene dissolved in the water unite with the oxygene if it had been lodged in it for several months? We design to make a long series of experiments upon this subject. If the hydrogene and oxygene contained in the water could combine in it, we should be able more readily to conceive how the hydrogene gas which rises from the surface of the earth is not to be discovered either in the air which surrounds us, or in the high regions of the atmosphere into which we have ascended. We must recollect upon this subject that having carefully examined rain-water, in order to discover hydrogene in it, we have ascertained that the air disengaged from this water did not contain a quantity of it amounting to $\frac{3}{1000}$. We shall repeat these experiments upon rain that has fallen in different seasons, and particularly during storms.

River water placed in contact with mixtures of gases has in general acted less upon the mixtures of oxygene

and azote than those of oxygene and of hydrogene. This result will appear the less surprizing if we take a general view of what takes place in these phenomena.

We find that water has a continual tendency to place itself in a state of equilibrium with the gases presented to it. If we present oxygene to it, it abandons azote. If we place it in contact with azote, it abandons oxygene. If we present to it a mixture of oxygene and hydrogene, it absorbs a part of these two gases, and replaces them with azote. It every where tends to modify the proportions of the air which it holds in solution according to the nature of the gases which are presented to it. Now the water of the Seine being charged with a mixture of azote and of oxygene, it seems natural that it should excite more action upon a mixture of hydrogene and oxygene than upon that of azote and oxygene, which is analogous to the air which it holds in solution. In order to obtain an accurate idea of these phenomena, we shall make experiments upon water recently deprived of air, by charging it with different gases both pure and mixed, and examining the action of this water after a long space of time; for frequently it is not till after a long repose that nature is enabled to overcome the obstacles which oppose themselves to the action of the affinities.

Here we shall conclude the account of the inquiries into which we have been engaged for several months past. The more extensive the field is which we purpose to explore, the more strongly we feel the imperfection of the work which we now present to the public; but this sentiment, so far from discouraging us, will only render us the more assiduous in interrogating nature, in order that we may carry these researches to a higher degree of perfection.

TABLE

TABLE

Representing the Analysis of the Air.

Days when the Air was collected.	Temperature expressed in Degrees of the centigrade Thermometer.	State of the Atmosphere.	Absorption proceeding from the Inflammation of 200 of Air and 200 of Hydrogene.	Quantity of oxygene contained in 100 Parts of Air.
Brumaire 26	7°.3	Sky overcast; wind E.	126,0 126,0	21,0 21,0
27	4,5	Sky overcast; wind E.S.E.	126,0 126,0	21,0 21,0
28	4,7	Light rains; very high wind S.W.	126,0 126,0	21,0 21,0
29	10,0	Light rain; wind S.	126,0 126,5	21,0 21,1
30	12,5	Sky overcast; wind S.W.	126,8 126,0	21,2 21,0
Frimaire 1	6,7	Sky cloudy, slight rain; wind S.W.	126,0 126,0	21,0 21,0
2	1,5	Cloudy sky; wind W.	126,0 126,0	21,0 21,0
3	8,5	Rain; wind S.	126,3 126,5	21,0 21,1
4	10,6	Sky overcast; wind S.W.	126,2 126,6	21,0 21,1
5	3,3	Cloudy sky; wind E.	126,5 126,0	21,1 21,0
6	—1,6	White frost; wind N.	126,0	21,1
7	—1,3	Snow; wind N.	12,65	21,0
10	—4,1	Fog; wind N.N.E.	126,0	21,0
12	—2,3	Cloudy sky, mists; wind E.	186,5	20,9
14	4,2	Rain; wind S.	126,0	21,0
16	3,1	Thick fog	126,0	21,0
22	9,6	Rain; wind S.S.W.	126,0	21,0
28	—2,2	Sky overcast; wind N.E.	126,0	21,0
Nivose 2	1,0	Hoar frost, thick fog; wind S.E.	126,0	21,0

Intelligence

Intelligence relating to Arts, Manufactures, &c.

(Authentic Communications for this Department of our Work will be thankfully received.)

Mountain Soap.

M. BUCHOLZ has analyzed a fossil, termed by Werner *Argilla Saponiformis*, in German *Bergseiffe*. (Mountain Soap.)

This mineral, which is very scarce, having hitherto been found only in Poland and in Cornwall, has lately been discovered also at Artern, in Upper Saxony.—According to M. Bucholz's analysis, a thousandth part of this substance consists of silex 440, alumine 265, oxyd of iron 80, water 205, lime 5, with 5 of loss.

List of Patents for Inventions, &c.

(Continued from Page 400.)

PATRICK WHYTOCK, of Liverpool, in the county of Lancaster, Merchant; for an improvement in the manufacture of piece goods, composed of cotton, of flax, or of hemp, or of any mixture or mixtures of two or more of these articles, by which such goods will resist the rotting action of wet or moisture much better than similar fabrics manufactured by the methods in common use.
Dated March 8, 1806.

JOHN CURR, of Sheffield Park, in the parish of Sheffield, in the county of York, Gentleman; for a method, different from any that has hitherto been invented or known, of spinning hemp for making of ropes or cordage.
Dated March 8, 1806.

RICHARD

RICHARD WILLCOX, of the parish of St. Mary, Lambeth, in the county of Surrey, Merchant; for certain machinery for glazing and graining leather, now usually performed by hand. Dated March 8, 1806.

EDWARD DAMPIER, EDWARD JACKSON, and JAMES SHACKLETON, of Primrose-street, in the city of London, Manufacturers; for certain machinery for rasping, grating, or reducing into small parts or powder, such woods, drugs, and other substances, for the use of dyers and others, as are not easily to be pulverized by mere percussion. Dated March 12, 1806.

MICHAEL LOGAN, of Paradise-street, in the parish of Rotherhithe, in the county of Surrey, Engineer; for an entire new system of marine, fort, and field artillery. Dated March 13, 1806.

CHARLES ROBERT WEST, of Plough-court, Fetter-lane, in the city of London, Optician, and WILLIAM BRUCE, of King's Head-court, Shoe-lane, in the city of London, Optical-turner; for improvements in day or night telescopes, whereby the same will be rendered more portable than they now are. Dated March 18, 1806.

HENRY GOVE CLOUGH, of Norton-steet, in the parish of St. Mary-le-bone, in the county of Middlesex, Surgeon; for improvements in the instruments or apparatus commonly called trusses, which are used for compressing and supporting such parts of the human frame as are or may be ruptured or disposed to protrude. Dated March 21, 1806.

FRANCIS PLACE, of Charing-cross, in the parish of St. Martin in the Fields, in the county of Middlesex, Taylor and Mercer; for improvements in locks for muskets, pistols, fowling pieces, carriage guns, and every species of fire-arms. Dated March 21, 1806.

RICHARD

RICHARD OTTLEY, of Myrtle-hill, near Caermarthen, in Carmarthenshire, Esq. and JAMES JEANS, of Portsmouth, in the county of Hants, Ship Builder; for improvements in chain-pumps, in the mode of working the same, and in the wells for receiving such pumps; whereby much manual labour will be saved.
Dated March 21, 1806.

JOSEPH HINCHSLIFFE, of Dumfries, in that part of the United Kingdom called Scotland, Cutler and Surgoen's instrument-maker; for a method of manufacturing elastic spring trusses for ruptures or rupture bandages.
Dated March 26, 1806.

BRACEY CLARK, of Giltspur-street, in the city of London, Veterinary Surgeon; for improvements upon horse-shoes. Dated March 26, 1806.

QUINTIN M'ADAM, of Anderston, near the city of Glasgow, in the county of Lanark, in that part of the United Kingdom called Scotland, Manufacturer; for an improved method of dressing yarns for weaving, by means of a new and useful machine. Dated March 26, 1806.

WILLIAM PARR, of Bermondsey New-road, in the county of Surrey, Gent. RICHARD BEVINGTON, of Gracechurch-street, in the city of London, Merchant, and SAMUEL BEVINGTON, of Grange-road Bermondsey, in the said county of Surrey, Leather-dresser; for a machine for splitting hides, skins, pelts, or leather, in an improved manner. Dated March 26, 1806.

END OF THE EIGHTH VOLUME, SECOND SERIES.

ERRATUM.

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